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QUARTERLY JOURNAL OF THE
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DECEMBER 1995

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ARTICLES

American Political Science Review Vol. 89, No. 4 December 1995

A MODEL OF MUDDLING THROUGH

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As arguments about the effectiveness of "muddling through" have proven frustratingly inconclusive, incrementalism—once a major approach to the study of boundedly rational policy processes—has gone dormant. In an attempt to revitalize the debate, I present a formal model of muddling through. The model, by clarifying the logical structure of the informal theory, presents a clearer target for criticism. More importantly, it establishes numerous deductive results. First, some of Lindblom's less controversial conjectures—about the benefits of seriality (repeated attacks on the same policy problem) and redundancy (multiple decision makers working on the same problem)—turn out to be correct if conflict across policy domains is absent or takes certain specified forms. But given other empirically reasonable types of conflict, even these claims are wrong. Second, the advantages of incremental (local) policy search (Lindblom's best-known and most controversial claim) turn out to be still less well founded: in many empirically plausible contexts the claim is invalid.

Old theories in political science rarely die; they usually just fade away. This has been incrementalism's fate. Even in hindsight, this was a curious end to incrementalism's intellectual trajectory. The basic ideas of "muddling through" were described in an extremely well known series of publications (Lindblom 1959, 1965; Braybrooke and Lindblom 1963): the 1959 essay has been reprinted in about 40 anthologies (see Lindblom 1979, 524); the two books are classics. And the theory also received its share of pointed criticisms (e.g., Arrow 1964; Boulding 1964; Dror 1964; Etzioni 1967; Goodin and Waldner 1979; Lustick 1980; Schulman 1975).

Yet neither Lindblom nor his critics were able to carry the day. In particular, Lindblom's claims regarding incrementalism's effectiveness have been neither established nor refuted. Consider, for example, the controversy over the benefits of changing the status quo only by small steps. Critics raised concerns that "incrementalism would tend to neglect *basic* social innovations as it focused on the short run and sought no more than limited variations from past policies" (Etzioni 1967, 387, emphasis original); that it would reinforce "the pro-inertia and anti-innovation forces prevalent in all human organizations" (Dror 1964, 155); and that it may embody "a complacent acceptance of our imperfections" (Arrow 1964, 588). Lindblom (1964, 1979) replied to this criticism, but it is fair to say that the arguments on both sides, though reasonable, are less than compelling. Thus what is perhaps the best known of Lindblom's claims—that making small policy changes is often superior to making radical ones—retains the status of a conjecture over 30 years after it was first proposed.

Or consider the issue of context. As Lustick reminded us, the debate over incremental versus synoptic strategies should be couched in terms of their relative effectiveness in different decisional contexts, for it is unlikely that one is better than the other in all circumstances (1980, 342). Braybrooke and Lindblom took this point seriously, devoting an entire chapter to analyzing the match between strategies and their appropriate contexts (1963, 61–79). But the status of these matches—which are themselves hypotheses—remains unclear. Braybrooke and Lindblom asserted that incrementalism was suited to situations involving low understanding (p. 78). However, Lustick hypothesized that incrementalism's relative utility "must be discounted to the extent that the complexity of an organization's task environment is non-decomposable" (1980, 346). In their criticism of this essay, Knott and Miller interpreted Lustick's claim to be inconsistent with Braybrooke and Lindblom's:

> The more a problem is characterized by nondecomposable complexity, the less likely it is that all the interaction effects among the parts of the problem will be understood. The less understood these complexities, then the more likely it is that synoptic decision making will fail, since synoptic decision making assumes a comprehensive theory capable of accounting for every relevant factor and interaction effect. Clearly, it is in the presence of little-understood complexities that incrementalism has a comparative advantage over synoptic decision making, if ever. (1981, 725–26)

Lustick's reply in the same volume continued the debate but did not resolve it. (That the debate did not seem to be making progress may be indicated by the fact that one finds fewer articles on incrementalism in the rest of the 1980s.) This is a frustrating and unsatisfactory state of affairs. Three-and-a-half decades after "The Science of 'Muddling Through' " was published, we should have a better grasp of the theory's central propositions.

My premise here is that this stagnation occurred primarily because both sides of the debate relied exclusively on informal reasoning.[1] It is very difficult to analyze a strategy's effectiveness without the aid of formal models, either closed form mathematical models or computer simulations.

The advantages of formalization can be seen by comparing the intellectual history of incrementalism with that of the other main research tradition on bounded rationality in political science, that of *satisficing* (Simon 1957). Even in ordinary English, the heuristic of satisficing is simpler than the heuristics of disjointed incrementalism. A satisficing target or aspiration level serves as a stopping rule in a search for alternatives: the decision maker ends the search once he or she encounters an option that exceeds the aspiration level, instead of trying to optimize by balancing the marginal costs and benefits of further search. This is clear even in ordinary language, but in addition, formalization has made the concept still more precise. Simon (ibid.) himself built formal models of satisficing. And though these were decision-theoretic, examining an active decision maker in a passive environment, it has proved possible to build on his foundations by constructing aspiration-based models that investigate the strategic interaction of several actors (Bendor, Mookherjee, and Ray 1993; Cyert and March 1963; Gilboa and Schmeidler 1993). Accordingly, the idea of satisficing is alive and well in current research on decision making. The contrast with the theory of incrementalism could not be sharper.

I shall try to ameliorate this problem by formalizing some of Lindblom's central ideas. The relation between the ensuing formal model and the literary theory of incrementalism is a complex one. On the one hand, the model is not a full-blown formalization of the complete strategy of muddling through. That would be much too ambitious. As Arrow noted in his review of Braybrooke and Lindblom's *Strategy of Decision*, "It is not easy to give a simple statement of their strategy, since, by its nature, it is intended to be flexible and adaptable to differing situations" (1964, 586). Something hard to describe must be hard to formalize as well.

On the other hand, it turns out that in some respects the model is *more* complex than the informal theory: to construct a logically complete model, one must make assumptions about the nature of judgment in decision making (e.g., Does judgment vary over time?), about which Lindblom's original formulation said little or nothing. The number and variety of these assumptions (stationary versus nonstationary judgment, weak versus strong competence) indicate that in contrast to Arrow's remark, in some ways the informal theory was too simple, not too complex.

Indeed, by forcing us to confront questions that may have gone completely unnoticed in an informal analysis, formalization can provide a major intellectual benefit. More generally, the formal model can clarify the overall logic of Lindblom's informal theory—not only by filling in holes but also by making obvious exactly which claims are assumptions and which are derivations and by rigorously showing which consequences do indeed flow from basic assumptions.

With the help of a mathematical model, we can discover when a local or incremental search for alternatives is superior and when a bolder approach is better and how improved understanding affects different search strategies—the two controversies noted above. We are also able to address several less controversial claims of Lindblom's theory: the purported advantages of "seriality" (repeated attacks on the same policy problem) and of redundancy (multiple decision makers working on the same problem). So that we do not give away too many of the results, let us note here only that incremental search fares much worse than Lindblom had conjectured and that although seriality and redundancy are found to be often advantageous, under certain well-specified (and not implausible) conditions even they do more harm than good. Thus the critiques of Etzioni and Dror were well focused: though the uncontroversial claims about seriality and redundancy do not hold up perfectly, they do better than the controversial one of local search.

Because Lindblom's ideas are complex, we take a leaf from his own work and build the models up serially, beginning with the simplest. Thus the first model focuses on a single decision maker. Even in this simple context the model captures several essential features of muddling through: it represents a myopic and fallible decision maker, who chooses by comparing the status quo to a few new options, and who tries in a groping manner to choose objectively better policies over poorer ones. The decision maker's limitations are partially eased by the fact that he or she makes and remakes policy in "an endless series"—what Lindblom calls *seriality*. Following his emphasis on amelioration, the decision maker is only interested in whether a new alternative is better than the status quo.

A simple choice process underlies all the models. In period 1, the decision maker confronts a pair of alternatives, chooses one and discards the other. In period 2, a new policy is generated from the available pool of possibilities. It is then pitted against the status quo (last period's selection), and a new choice is made. This process is repeated indefinitely. We can then study the impacts of seriality (how the status quo's quality evolves over time), of incremental versus bolder search and how changes in policy understanding affect the relative utility of incrementalism.

The second model introduces an important institutional method for compensating for the cognitive limits on individual agents—multiple decision makers. Here we are interested only in the sheer effect of increasing the number of choosers. We suppress conflict of interest, assuming instead that they have the same objectives. The third model allows the decision makers to have conflicting objectives and analyzes the path of the status quo over time. We thus build, step by step, to a dynamic model of boundedly rational decision makers who have incomplete information and conflicting objectives. Such models are so complex that simulation is often the only way to solve them (e.g., Bendor and Moe 1986; Kollman, Miller, and Page 1992). Nevertheless, even the last model yields analytical results.

THE MODEL WITH A SINGLE DECISION MAKER

I shall first focus on a single decision maker. Though this player makes a sequence of choices over time, we do not endow him or her with information about the pool of feasible alternatives.[2] Instead, the player simply compares the status quo with a new alternative, chooses the one that he or she believes is superior, and throws away the other one. The process then continues to the next period.

Concepts and Notation

In every period, a new option will be generated from a set of possible types of policies; this generation process will be described shortly. For all but one of the results, the set of possible policies is assumed to be unbounded; the different kinds will be labeled $\{\ldots, -2, -1, 0, 1, 2, \ldots\}$. Higher integers denote objectively better policies.

Because the decision maker does not know for sure which type of policy he or she is confronting, his or her key property is competence—the probability of choosing the better of two options. In theories such as Condorcet jury models in which decision makers must make one pairwise choice (guilty or not guilty), with neither option considered the status quo, a decision maker's competence is completely described by a single number, a probability that is presumably in $(\frac{1}{2}, 1)$. Naturally, if jurors err more often than fair coins would, the latter could replace the former. Hence it may be reasonable to use this assumption in a first pass at the problem. Moreover, the assumption has the virtue of simplicity.

It is not, however, a fully satisfactory cornerstone for a model of muddling through for three reasons. First, the decision maker makes choices repeatedly. Hence time could matter: the decision maker might, for example, become more competent with experience. Second, because there can be more than two types of alternatives, the probability of being correct might vary across pairs of options. Suppose, for example, that alternatives can be poor, mediocre, or excellent. Compare two choice situations: in one, the status quo is poor and the new proposal is mediocre; in the other, the status quo is poor and the new option is excellent. Presumably it is easier to determine that (an objectively) excellent new alternative is better than (an objectively) poor status quo than it is to conclude that a mediocre option is better than a poor one. Hence, if the status quo is poor, the probability of accepting a new alternative that is excellent should exceed the probability of accepting one that is only fair.[3] Third, models that are status-quo-based must allow for the possibility that the status quo policy has special significance in the decision maker's mind, a possibility that is especially salient in theories of incrementalism. Moreover, recent work (Kahneman, Knetsch, and Thaler 1990) has provided empirical support for the idea that the status quo enjoys a special significance.[4]

Given these theoretical and empirical considerations, one might want to hesitate before embracing the assumption that the chance that a decision maker will choose correctly always exceeds one-half. And any model of muddling through must make *some* kind of assumption about the effects of time, multiple pairwise comparisons, and the status quo. These three issues will be addressed by the definitions that follow. First, some useful notation. Let $p_{i,j;t}$ denote the chance that given a status quo i and a new option j at date t, the decision maker will choose the new alternative over the status quo. (Here, i and j are integers denoting different quality policies.) If $j > i$, then j is objectively better than i; hence in this case $p_{i,j;t}$ is the probability of deciding correctly. If $j < i$, then $p_{i,j;t}$ is the probability of making the wrong choice between i and j.

As a benchmark case, let us reconsider the simple Condorcet assumption that the decision maker has a constant probability, $P > \frac{1}{2}$, of being correct. If we embed this premise in a model of muddling through, we shall see that it makes certain presuppositions about the effect of time and of multiple pairwise comparisons and about the impact of the status quo.

Time. Because in the benchmark case a decision maker has a fixed and constant probability of being correct, judgment is unaffected by time. I shall say that judgment is *stationary* if $p_{i,j;t}$ equals some constant $p_{i,j}$ in every period, for every status quo i and every new option j. Judgment is nonstationary if this condition does not hold. Some of the results that follow allow for nonstationary judgment.

The Effect of the Status Quo. In the benchmark case, the status quo is unimportant. In a comparison between alternatives i and j (where, e.g., $i < j$), it does not matter which one is the status quo: the chance of choosing correctly is P in either case. Thus $p_{i,j;t}$, which (given $i < j$) is the probability of correctly discarding an inferior status quo, equals $1 - p_{j,i;t}$, the chance of correctly keeping a superior status quo. Hence the decision maker is not biased for or against the status quo. I shall say that judgment exhibits *status quo insensitivity* if $p_{i,j;t} = 1 - p_{j,i;t}$. Judgment is sensitive to the status quo if this condition does not hold.

Multiple Pairwise Comparisons. With the benchmark assumption, every choice situation looks the same in that, in any given period, the chance of moving "forward" (selecting an objectively better new alternative) is always P and the chance of moving "backward" (accepting an objectively poorer one) is always $1 - P$. In this, the simplest possible type of judgment, the agent can only distinguish *superior* from *inferior* options. Any new option that is better than the status quo looks like any other superior new alternative, no matter how much better one or the other is. Similarly, all inferior new proposals look the same. I shall call this property *crude judgment*. Formally, judgment is crude if for all $h < i$ and all $j < k$, $p_{h,i;t} = p_{j,k;t}$ and $p_{i,h;t} = p_{k,j;t}$. As a mnemonic notation, when judgment is crude let f_t denote the probability of moving forward from any status quo in period t (i.e., $f_t \equiv p_{i,j;t}$ for all $i < j$) and let b_t denote the probability of moving backward from any status quo in that period ($b_t \equiv p_{i,j;t}$ for all $i > j$).

Judgment can be more discriminating if it is *homogeneous* across status quos. Judgment that is homogeneous can take into account the difference between a new proposal that is vastly better than the status quo and one that is only a bit better and likewise between a new option that is barely worse than the status quo and one that is a disaster. Formally, all that is required for judgment to be homogeneous across status quos is that for all integers k, $p_{i,i+k;t} = p_{j,j+k;t}$. For example, the probability of going from a status quo of 10 to a new alternative worth 12 must equal the chance of going from a status quo of 20 to a new alternative worth 22. Note that crudeness is a special case of homogeneity; judgment can be homogeneous without being crude. (For example, if judgment is homogeneous but not crude, then the chance of discarding a status quo of 10 could be greater if the new alternative were worth 80 than it would be if the new option were worth only 12. Crude judgment requires that those probabilities be the same.) Judgment is heterogeneous if it is not homogeneous.

Finally, one must make some assumption about the agent's competence. A decision maker will be called *weakly competent* if for every status quo i, (1) $p_{i,j;t} \leq p_{i,j+1;t}$ for all j, $j + 1 \neq i$, and (2) $p_{i,i+1;t} > p_{i,i-1;t}$. Property 1 is a natural assumption: inferior new options cannot be more likely to induce an agent to discard a given status quo than are better ones. Together, the two properties ensure that even a weakly competent decision maker is more likely to discard a fixed status quo in favor of a superior new policy than to do so in favor of an inferior new one.

A decision maker will be called *strongly competent* if property 1 holds and, in addition, $p_{i,j;t} > \frac{1}{2}$ whenever $i < j$ and $p_{i,j;t} < \frac{1}{2}$ whenever $i > j$. Thus a strongly competent agent has more than a 50% chance of selecting correctly, no matter what kind of choice situation confronts her. Weak competence does not imply this level of accuracy in all situations. Clearly, then, strong competence implies weak competence but the converse does not hold.

Weak competence permits the decision maker to be biased toward (or, less plausibly, against) the status quo. Thus even if $i < j$, $p_{i,j;t}$ need not exceed one-half; indeed, it could be close to zero. If it is, then the player is very biased toward the status quo. Hence if $h < i$, then $p_{i,h;t}$ must be still closer to zero. Even in the family of models of bounded rationality, weak competence is a very mild assumption. A bias favoring the status quo (plus even a bit of evaluative accuracy) should imply that the decision maker is more likely to move forward from the status quo than backward. In contrast, strong competence implies that the decision maker cannot be predisposed in favor of the status quo if the latter is inferior to a new proposal.

We can now step back and summarize. The benchmark assumption—imported from Condorcet jury models—that a decision maker has a constant probability of choosing correctly, presumes strong competence and judgment that is crude, stationary, and insensitive to the status quo.

The generation of new options is assumed throughout to be Markovian: given a status quo of i in period t, there is a conditional probability $q_{i,j;t+1}$ of producing alternative j as the challenger in the following period. Consistent with Lindblom's position on the constraints on policy design, it is always assumed that in any period only finitely many new types of policies are possible.

As with choice, one must specify the effect of time and the role of the status quo. We will say that alternative generation is *stationary* if $q_{i,j;t} = q_{i,j}$. Thus the chance of generating an option of type j in period t depends only on what was the status quo in $t - 1$, not on the date, t. If this condition does not hold, the process is nonstationary.

Policy generation is *homogeneous* across status quos if $q_{i,i+k;t} = q_{j,j+k;t}$ for all k. That is, if in a given period one normalized two different status quos to zero, the new alternatives associated with each would be distributed identically. Thus, taking into account the value of each status quo, the distribution of new alternatives looks the same. If this condition does not hold, the process is heterogeneous. (The best-known Markov chains, such as random walks, are both stationary and homogeneous.)

Because the process must start somehow, there is a "dummy" period—period 0—in which an alternative is generated but because there is nothing to compare it to, this option is the agent's de facto choice in this period. Thus in period 0 there is an initial probability vector, $[q_1, \ldots, q_M]$, that specifies the unconditional probability of drawing any particular type of alternative. The real choice process begins in earnest in period 1.

Combined, the assumptions about generation and choice create a Markov chain. A probability transition matrix for this type of stochastic process, with stationary judgment and policy generation, is shown in Figure 1.

The state of this chain at any date is the status quo policy. Let V_t denote the value or quality of the status quo policy in period t. Because V_t is produced by a choice process marked by errors in judgment and randomness in the generation of alternatives, it is a random variable. V_t is our primary object of interest.

To avoid the triviality of the choice process being "stuck" in a particular state, e.g., $q_{i,i} = 1$ (so that if the agent ever chooses a policy of type i, no other type of alternative will ever be produced for consideration), I shall assume that it is always possible to generate new options that are better or worse than the status quo. Hence in every period there are strictly positive integers j and k such that for every status quo i, $q_{i,i-j;t} > 0$ and $q_{i,i+k;t} > 0$.

Given these definitions, alternative generation must be either homogeneous or heterogeneous and

FIGURE 1

Probability Transition Matrix (Stationary Transition Probabilities)

Status quo in t+1

Status Quo in *t*

	......	*i*	......	*j*	......	*k*	......
⋮							
i		$1-\sum_{h\neq i} q_{i,h}p_{i,h}$		$q_{i,j}\,p_{i,j}$		$q_{i,k}\,p_{i,k}$	
⋮							
j		$q_{j,i}\,p_{j,i}$		$1-\sum_{h\neq j} q_{j,h}p_{j,h}$		$q_{j,k}\,p_{j,k}$	
⋮							
k		$q_{k,i}\,p_{k,i}$		$q_{k,j}\,p_{k,j}$		$1-\sum_{h\neq k} q_{k,h}p_{k,h}$	
⋮							

Note: $q_{i,j}$ denotes the probability that given a status quo of *i*, a new alternative, *j*, will be generated; $p_{i,j}$ denotes the probability that given a status quo of *i*, the decision maker will choose the new option, *j*.

either stationary or nonstationary. Similarly, judgment must be either sensitive or insensitive to the status quo, either homogeneous or heterogeneous, and either stationary or nonstationary. Because each pair is exhaustive, whenever one of the following results obtains under either (say) stationary or nonstationary alternative generation, then the statement of that result will not refer explicitly to that property.

The Effect of Seriality

We are now ready to examine how the quality of the status quo changes as time unfolds. The first theorem focuses attention exclusively on how the agent's judgment affects the value of the status quo policy over time. This is achieved by holding the generation of new alternatives probabilistically constant: it is assumed that the distribution of new proposals is independent of the status quo (i.e., for all *i* and *j*, $q_{i,j;t}$ just equals some constant $q_{j;t}$) and that this process is stationary: $q_{j;t} = q_j$. (Given stationarity and the premise, used throughout, that only finitely many new alternatives are possible in any period, it follows that the policy space in theorem 1 must be finite. The possible alternatives are labeled $\{1, \ldots, M\}$, where higher numbers denote superior policies.)

Given these assumptions, the chance of generating, say, the best possible alternative, *M*, remains the same at every date. Consequently, if $p(V_t = M)$ increases over time, we know that *all* of this improvement in V_t must be attributed to the agent's evaluations of alternatives, not to how those options are produced. Theorem 1 shows that this improvement is marked.

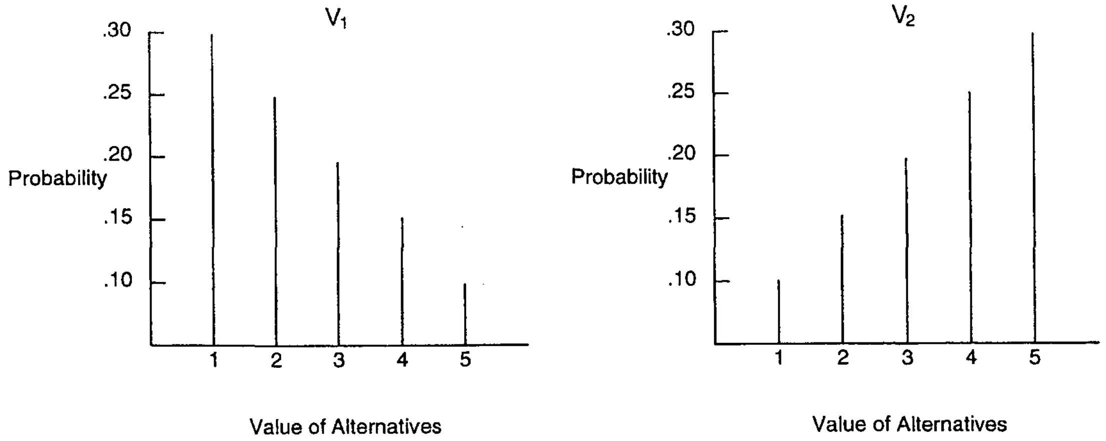
To analyze the change in V_t, we use the criterion of stochastic improvement. Suppose V takes on values $\{m, \ldots, M\}$ and V' has support $\{m', \ldots, M'\}$, where $m \leq m'$ and $M \leq M'$. Then V' is stochastically better than V if $p(V' > v) > p(V > v)$ for $v = m, \ldots, M' - 1$. If $p(V' > v) \geq p(V > v)$ for $v = m, \ldots, M' - 1$, we shall say that V' is weakly stochastically better than V. Thus stochastic improvement means shifting probability weights toward better outcomes (see Figure 2). It is worth observing that if V_t gets stochastically better over time, then $E[V_t]$ must also rise. The converse does not hold: the average may increase without stochastic improvement. Hence the latter is a stronger criterion than the former. (Theorem 1 and all other theorems are proven in the Appendix. The proofs of all propositions and lemmas are available on request from the author.)

THEOREM 1. *If the decision maker is weakly competent, judgment is crude and stationary, and the distribution of new proposals is independent of the status quo, then* V_t *improves stochastically with time.*

Note that *no* structure has been imposed on the distribution of new options, which may be multi-

FIGURE 2

V_2 Is Stochastically Better Than V_1



peaked, or sharply skewed toward poor options, or the like. Regardless of the shape of this distribution, more time means stochastically superior decisions. Of course, the fact that the decision maker is weakly competent is absolutely essential to this result. Indeed, given the other assumptions, weak competence is a *necessary* condition for V_t to improve stochastically. If the decision maker were more likely to select an inferior new policy over the status quo than to choose a superior new one over the status quo, then V_t would *degrade* stochastically over time.

Theorem 1 is useful as a baseline because we know that none of V_t's improvement is due to anyone learning how to generate better options. But empirically we do expect the processes of generation and evaluation to be intertwined. Accordingly, all of the results that follow allow for this interdependence.

The process postulated for the next theorem is extremely general: as before, the agent need only be weakly competent, but now his or her judgment can be either stationary or nonstationary. Moreover, the generation of alternatives may depend upon the status quo, and in ways that may be both nonstationary and heterogeneous.

The bad news is that we must give up the criterion of stochastic improvement in this environment. The culprit is partly the generality of alternative generation. A policy process that admits such generality in how new policies are produced can reach new (heretofore unreached) lows, as well as new highs. (For example, suppose that the status quo in period 0 must equal zero. New proposals in period $t + 1$ equal ± 1 of period t's status quo. This random walk can take on a value of -1 in period 1, of -2 in period 2, and so on; hence stochastic improvement is unattainable.) We therefore fall back on $E[V_t]$—the average value of V_t—as the normative standard.

The most important assumption of theorem 2 is that the new proposals, given a status quo, are on average at least as good as that status quo. To write this concisely, let Q_{t+1}, a random variable, denote the value of new alternatives at the start of period $t + 1$; hence $E[Q_{t+1}|V_t = i]$ denotes the average value of new alternatives in $t + 1$, given that i was the status quo in t.

THEOREM 2. *If* $E[Q_{t+1}|V_t = i] \geq i$ *and the decision maker is weakly competent, then* $E[V_t]$ *increases monotonically over time.*

This result can be well understood if we consider an important special case. If Q_t is symmetrically distributed around the status quo, the burden of improving the average is thrown entirely on the evaluation process. Because the agent is weakly competent, he or she must be at least a tad more likely to select better options than mistakenly to embrace weaker ones. Given that Q_t is symmetric around the status quo i, the decision maker is therefore more likely to progress to an alternative worth $i + k$ than to fall back to its symmetric twin worth $i - k$. Hence the average must rise.[5]

Incremental Search and Uncertainty

The best-known of Lindblom's claims is that making incremental policy changes is generally superior to making radical ones. To investigate this idea systematically we must first define a *less incremental policy process*. The intuitive idea is that incrementalism involves small policy changes, nonincrementalism, large ones (Braybrooke and Lindblom 1963, 62). For example, financing public schools completely by vouchers would be a big change; requiring that schools teach fourth-grade mathematics in a new way would be a more incremental change.

More abstractly, let there be a (possibly multidimensional) space that describes the key programmatic characteristics of different alternatives. A *search strategy* is simply a point in this space. The Euclidean distance, d, between the status quo and the search strategy measures the degree of nonincrementalism in programmatic change: the bigger d is, the more radical the attempted change. Thus, consistent with at least some versions of the informal theory, "we draw no sharp line between the incremental and the nonincremental; the difference is one of degree" (Braybrooke and Lindblom 1963, 64).

It is important to note that nonincremental search does not imply "comprehensiveness," in Lindblom's sense of the word. Only a small part of the policy space is investigated. And, as usual, only a finite number of new options are possible in any given period. Indeed, one could assume that *fewer* options are generated when policy design is radical. Thus nonincremental search, though not "local," remains "limited."

Having defined incremental search, we can now examine Lindblom's argument about how it is supposed to affect policy processes.[6] The basic idea is that nonlocal policy design creates more uncertainty. However, more than one type of uncertainty might arise. First, some distributions of new options will be riskier—hence more uncertain—than other distributions. (A precise definition of this idea will be given shortly.) Second, the evaluation of any *one* new alternative may involve more or less uncertainty.

It seems that Lindblom argued that *both* kinds of uncertainty increase as search becomes less incremental: the distribution of new policies becomes riskier, and evaluation becomes "noisier." Regarding the first interpretation, Braybrooke and Lindblom have said that decision makers who muddle through "focus on the increments by which the social states that might result from alternative policies differ from the status quo. . . . They are focused on incremental alteration of existing social states" (1963, 85). And the assertions of Etzioni (1967) and Dror (1964) about the "pro-inertia" tendencies of incrementalism indicate that critics, too, see the strategy's cautious approach as producing new alternatives that are not innovative (i.e., not risky). Regarding the second interpretation, Braybrooke and Lindblom asserted that "an analyst is often without adequate information, theory, or any other organized way of dealing systematically with nonincremental alternatives" (1963, 89); hence such options are typically "unpredictable in their consequences" (Lindblom 1959, 85).

Whatever were the original beliefs of Lindblom and his critics, both assumptions seem plausible. It is reasonable to assume that incremental search is less risky: nonincremental search should be more likely than more local search to generate extremely bad and extremely good policies. And evaluating a new alternative that differs greatly from the status quo should be ridden with more uncertainty than evaluating one that differs only marginally. What is less clear is whether these two types of uncertainty affect policy outcomes in the same direction. The next set of results show that in fact, *they do not*. Whereas noisier evaluation has the harmful effect anticipated by Lindblom, the increased riskiness of a set of new options is benign. To show these distinct effects, we analyze them separately. In the first subsection that follows, it is assumed that less incremental search makes the distribution of new alternatives riskier; there is no effect on evaluation. In the second subsection, it is assumed that more radical search makes evaluation noisier; there is no effect on the distribution of new options. Finally, we examine interactions between the distribution of new policies and their evaluation.

Incrementalism and Risky Sets of New Options. Recall that Q_t, a random variable, denotes the value of new alternatives at the beginning of period t. Let us then use Q'_t to refer to the value of new options generated by a more radical search process; Q_t, the value of those generated by a more incremental process. (Hereafter, variables with prime superscripts will always be associated with less incremental search.) The key assumption is that Q'_t's distribution is riskier or more uncertain than is Q_t's. To make this idea precise, call the distribution of Q'_t riskier than Q_t if, after normalizing them so that they have the same status quo x_0, $p(Q'_t > x) \geq p(Q_t > x)$ for all $x > x_0$ and $p(Q'_t < x) \geq p(Q_t < x)$ for all $x < x_0$, these inequalities being strict for at least one x bigger than x_0 and at least one x smaller than x_0. Thus Q'_t is more spread out around the status quo than Q_t is.

To avoid rigging the results either for or against incrementalism, we also require that Q'_t be a *mean-preserving spread* of Q_t: $E[Q_t|V_t = x_0] = E[Q'_t|V'_t = x_0]$. Thus, given the same status quo, the two processes produce equally valuable new alternatives, on average (see Figure 3 for an example). No other constraint is placed on the shapes of the two distributions.

We can now compare the average status quo under more versus less incremental processes. Perhaps surprisingly, it turns out that this kind of uncertainty is helpful.

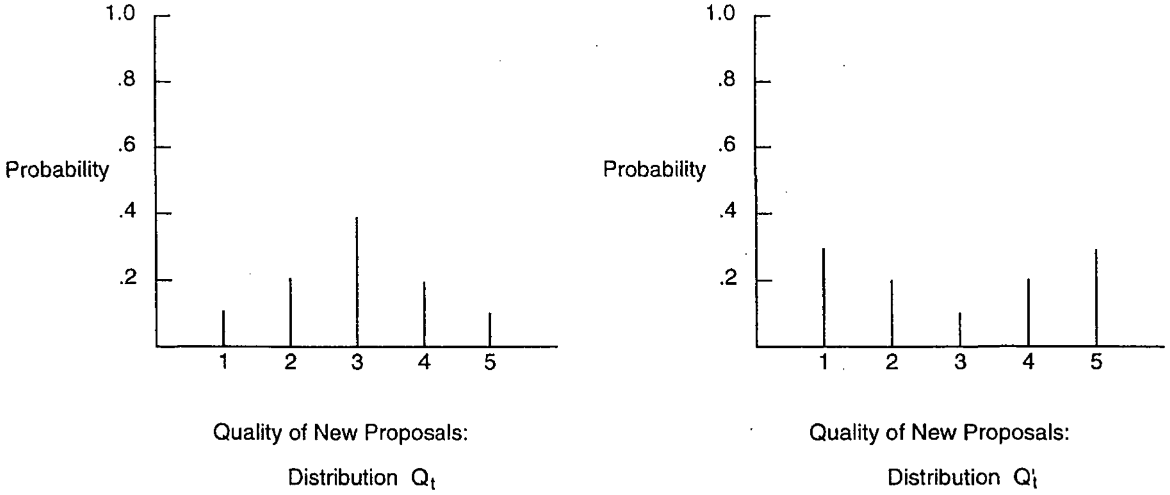
THEOREM 3. *Suppose search in the* V'_t *process is less incremental than in the* V_t *process, alternative generation in each process is homogeneous, the decision maker is weakly competent, and his or her judgment is crude. Then* $E[V'_t] > E[V_t]$, *for all* $t > 0$.

The intuitive explanation for this result is as follows. Suppose both incremental and nonincremental search generates new alternatives that are distributed symmetrically around the status quo, with the latter's distribution being more dispersed. Consider the extreme case in which there is no chance of moving backward ($b = 0$) by mistakenly accepting a new option that is inferior to the status quo. Hence the greater "downside" risk associated with radical search is irrelevant; all that matters is its greater "upside" risk, which is pure gravy. More generally, since a weakly competent agent is always more likely to move forward than backward ($f > b$), quantitatively the upside risk outweighs the downside risk.

It is straightforward to show that given the theorem's assumptions about alternative generation and judgment, weak competence is *necessary* for radical search to be superior to incremental search. If the decision maker is weakly *in*competent ($f < b$), then bold search hurts rather than helps. Thus theorem 3

FIGURE 3

Q_t' Is a Mean-preserving Spread of Q_t



Note: Theorem 3 assumes that Q_t', the riskier distribution of new alternatives, is produced by bolder search and that Q_t, the less risky distribution, is produced by more incremental search.

identifies the *minimal level of rationality* that is required in order to reject Lindblom's conjecture.[7]

Incrementalism and Noisy Evaluation. Here I assume that more radical search affects evaluation but not the distribution of new alternatives. To make this idea precise, let us posit that the *perceived* value of new options, $\tilde{Q}_t$, equals their true value plus some error term that represents "noise." Naturally, the decision maker who perceives the new option as being better than the status quo selects it; otherwise, he keeps the status quo.[8] Thus $\tilde{Q}_t = Q_t + \theta_t$, where θ_t is any continuous random variable. (It is not required that $E[\theta_t] = 0$. The evaluation of new options may be biased, which permits the decision maker to exhibit a status quo bias.) It is only required that θ_t can take on both positive and negative values, so that the agent can either overestimate or underestimate the value of a new alternative.[9]

As the notation suggests, θ_t may vary over time; consequently, the decision maker's judgment may be nonstationary. Within the same period, however, the error term produced by a given search strategy is constant. It is easy to show that this implies homogeneous judgment.[10] Observe also that this error structure does not imply even weak competence: only one of the attributes of a weakly competent agent—that $p_{i,j;t} \leq p_{i,j+1;t}$—is implied.

Paralleling the previous notation, let θ_t' denote the noise associated with evaluating a new policy generated by a more radical search process; θ_t, that associated with evaluating an incrementally generated option. We will say that θ_t' is *noisier* than θ_t if there is an x_0 such that $p(\theta_t' > x) \geq p(\theta_t > x)$ for all $x > x_0$ and $p(\theta_t' < x) \geq p(\theta_t < x)$ for all $x < x_0$, these inequalities being strict for at least one x bigger than x_0 and at least one x less than x_0.[11] To keep the comparison clean, we shall assume that θ_t' is a mean-preserving spread of θ_t. Hence this definition implies that evaluating radical options is riskier than evaluating more incremental options, with greater riskiness being formally defined in *exactly* the same way that greater riskiness was defined for the distribution of new alternatives. But here greater riskiness is indeed harmful.

THEOREM 4. *Suppose that search in the* V_t' *process is less incremental than in the* V_t *process and that less incremental search produces noiser evaluation. Alternative generation in each process is identical and is homogeneous. Then* V_t *is (weakly) stochastically better than* V_t', *for all* $t > 0$.

Searching more incrementally yields only weak stochastic improvement because less noisy evaluation does not always imply that judgmental accuracy increases. (Of course, the probability of being right can never *decrease* as noise falls.) Whether less noise means greater accuracy depends upon how the error terms, θ_t and θ_t', are distributed in relation to the values of the status quo and a given new alternative. The increased noiseness of θ_t' may occur in a judgmentally irrelevant region. For example, suppose in every period θ_t is distributed uniformly over $[-.4, .4]$ and θ_t' is uniform over $[-.8, .8]$. Then the judgment is flawless in each process, and V_t and V_t' will be distributed identically in every period.

There are, however, plausible restrictions on the error term that do imply that V_t is stochastically better than V_t' in the normal, strict sense. For example, if both θ_t and θ_t' are normally distributed, then judgment in the more incremental process will be strictly better than judgment in the less incremental one. This in turn implies that V_t is stochastically better than V_t' without qualification.

Interactions. We now examine interactions between the distribution of new alternatives and their evaluation. The first result focuses on how better policy understanding affects the value of bold search in different policy domains. In their typology of decision contexts, Braybrooke and Lindblom argued that incrementalism is more appropriate in situations in which there is little understanding (1963, 78; see also Knott and Miller 1981, 726; Braybrooke 1985, 926). One kind of understanding is manifested in accurate policy evaluation: the more consistently decision makers choose good alternatives over bad ones, the more understanding they display. Thus Braybrooke and Lindblom's argument implies that in domains where there is great understanding, decision makers can more easily afford to take the risks of radical change.[12]

As shown by theorem 3, the direct effect of less incremental search is to increase the average quality of the status quo, even when the agent is weakly competent and policy understanding is poor (f is less than one-half and close to b). This was unanticipated by Lindblom's argument. However, if Braybrooke and Lindblom's thesis about the match between strategy and policy context is correct, then the *relative* benefits of bolder search should rise as judgmental competence increases. For their specific conjecture about the interaction between incrementalism and policy understanding to be correct, it does not matter whether $E[V_t'] - E[V_t]$ is positive or negative; what matters is how this difference changes as f rises or b falls.

Interestingly enough, although Lindblom's hypothesis about incrementalism's direct effect on policy is invalid under the conditions of Theorem 3, his and Braybrooke's conjecture about the *interaction* between incrementalism and policy understanding turns out to be correct.

THEOREM 5. *If the conditions of theorem 3 hold, then the relative benefits of less incremental search,* $\mathrm{E}[\mathrm{V}_t'] - \mathrm{E}[\mathrm{V}_t]$, *increase as* f *rises or* b *falls.*

The intuitive explanation for this result is straightforward. If $f = 1$ and $b = 0$, then the decision maker gets the full benefit of the greater dispersion associated with bold search: superior new options are always chosen; weaker ones, always rejected. If $f = b$, one is just as likely to move backward as to move forward. Hence novelty in policy design is irrelevant. As f rises or b falls, more and more of the potential benefits of nonincremental search are realized.[13]

As suggested earlier, it is plausible that searching more radically will increase *both* kinds of uncertainty: the set of new policies will become riskier and the evaluation of any one alternative will become noisier. The final two results of this section examine contexts in which both effects occur. Because this is analytically more difficult, we are obliged to make more restrictive assumptions. Therefore we cannot allow θ to be any continuous random variable; instead, here it is distributed normally with mean zero and variance σ^2, and is stationary as well. Thus the noisiness of evaluation is parameterized by σ^2. We will assume that σ^2 increases as search becomes more radical. Thus the evaluation of a given new alternative depends upon the distance, d, between the status quo and the search strategy that generated the new option. Specifically, $\sigma^2 = d^\alpha$, where α is some positive constant. Thus α, the focus of the next result, parameterizes how well incremental search strategies reduce evaluation uncertainty, compared to nonincremental strategies.

As a further simplification, we shall dichotomize the search strategies. Instead of comparing continuous variation in search, as in theorems 3–5, for the next two results we will use categorical comparisons. As a normalization, if $d < 1$, call the search *incremental*; if $d > 1$, *nonincremental*. Then propositions 1 and 2 will compare a fixed incremental strategy—a particular d that is less than one—with a fixed nonincremental strategy—a d' greater than one. Thus to keep the analysis tractable, these two results will not make comparisons between two incremental strategies ($d < d' < 1$) or between two nonincremental ones ($d' > d > 1$).

Given the nature of this comparison, observe that a very large α represents a world in which being incremental during policy design gives the decision maker a big advantage in the policy evaluation phase, because $\sigma^2 = d^\alpha$ crunches toward zero as α increases (since $d < 1$), whereas $\sigma^2 = (d')^\alpha$ blows up (since $d' > 1$).[14] A value of α close to zero represents a world in which incrementalism during policy design gives very small leverage during policy evaluation.

Now for the result. Here we focus on a fixed pair of search strategies, d and d'. Because $d' > d$, the distribution of new alternatives generated by d' is, as before, riskier than the distribution generated by d. But now evaluation is affected as well.

PROPOSITION 1. *Suppose alternative generation is homogeneous and stationary and let* Q_t' *be riskier than* Q_t. *Let* $\tilde{\mathrm{Q}}_t = \mathrm{Q}_t + \theta$, *where* $\theta \sim \mathrm{N}(0, \sigma^2)$, *with* $\sigma^2 = \mathrm{d}^\alpha$; *hence the decision maker is strongly competent, with judgment that is homogeneous, stationary, and insensitive to the status quo. Then there exists an* $\alpha^* > 0$ *such that for all* $\alpha < \alpha^*$, $\mathrm{E}[\mathrm{V}_t] < \mathrm{E}[\mathrm{V}_t']$, *and for all* $\alpha > \alpha^*$, $\mathrm{E}[\mathrm{V}_t] > \mathrm{E}[\mathrm{V}_t']$, *for all* $\mathrm{t} > 0$.

A parallel result can be established for increasing degrees of riskiness. Again focus on a fixed pair of search strategies: $d < 1$ and $d' > 1$. We represent the effects of local versus nonlocal search as follows. Assume that new alternatives are homogeneously generated and distributed symmetrically around the status quo. For any status quo x_0, let $x_0 - m$ denote the smallest value of Q_t, and $x_0 + m$, the largest value; m is a strictly positive integer. Because $d' > d$, we will assume that the smallest and largest values of Q_t' are more extreme than those of Q_t. Specifically, let $x_0 - m - k$ denote Q_t''s smallest value, and $x_0 + m + k$ its largest. The parameter k is determined as follows. For any positive x, let $[x]$ denote the smallest integer that exceeds x. If x is a positive integer, then $[x] = x$. Then for a fixed $\beta \geq 0$, $k = [d^\beta] - 1$. Thus, if the search is incremental ($d < 1$), then $k = 0$ for any value of β. If search is nonincremental ($d > 1$) and β is strictly positive, then k is always at least 1.

We must also specify how variations in β (for a fixed search strategy d) and variations in d (for a fixed β) affect Q's complete distribution, not just its extreme values. Suppose $\beta_1 < \beta_2$. Given a fixed search strategy d, let $k_1 = [d^{\beta_1}] - 1$ and $k_2 = [d^{\beta_2}] - 1$ and let Q_1 and Q_2 be stationary distributions of new alternatives associated with β_1 and β_2, respectively. To keep matters simple, we will make the following assumptions. If, due to the rounding occasioned by $[\cdot]$, $k_1 = k_2$, then Q_1 and Q_2 are distributed identically. If $k_1 < k_2$, then Q_2 is a mean-preserving spread of Q_1. Thus, if β becomes sufficiently bigger so as to make the biggest and smallest values of Q more extreme, it makes Q itself riskier. If the change in β is insufficient to alter Q's extreme values, then the entire distribution of Q remains unchanged as well. In general, then, because the parameter k is (weakly) increasing in β, changes in β reflect how much dispersion in new options one gets for a given amount of nonlocal search.

We make analogous assumptions about the impact of changes in d on Q's distribution. Suppose for a fixed β we have $d_1 < d_2$. If $k_1 = k_2$, then Q_1 and Q_2 are distributed identically; if $k_1 < k_2$, then Q_2 is a mean-preserving spread of Q_1.

Finally, to ensure that a riskier distribution puts a nonnegligible increased weight on extreme values, it is assumed that there exists an $\varepsilon > 0$ such that for all Q'_t (i.e., no matter how large k is), if $p(Q'_t = x) > 0$ then $p(Q'_t = x) \geq \varepsilon$.

PROPOSITION 2. *Assume the conditions of proposition 1, with a fixed* α *(hence fixed* σ^2*'s, given a fixed* $d < 1$ *and a* $d' > 1$*). Suppose the above assumptions on the distributions of* Q *and* Q′ *hold. Then, apart from an unimportant knife-edge case (see Bendor 1995), there exists a* $\beta^* \geq 0$ *such that for all* $\beta \geq \beta^*$, $E[V'_t] > E[V_t]$ *and for all* $\beta < \beta^*$, $E[V'_t] < E[V_t]$, *for all* $t > 0$.

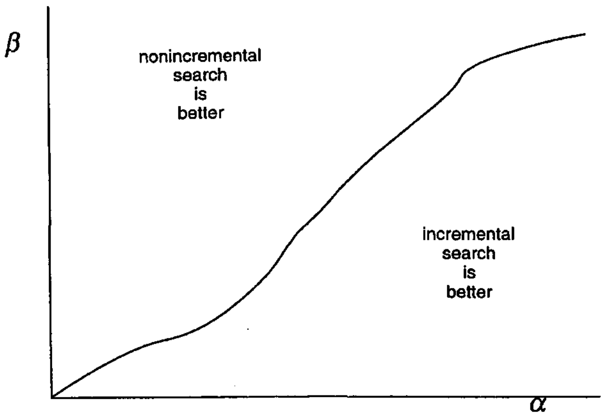
Propositions 1 and 2 together imply that if making the search more radical simultaneously produces *both* more dispersed alternatives and noisier evaluations, then the parametric space is partitioned into two regions (Figure 4). In one region, nonlocal search makes evaluation so difficult that this effect outweighs the benefits of riskier distributions of new alternatives (α is high, β is low). In the other region, the balance of effects is the opposite: evaluating unusual programs is not as hard, and nonincremental search yields much riskier distributions of options (α is low, β is high).

MANY DECISION MAKERS, SAME GOALS

A single, boundedly rational decision maker can only do so much, even if he or she exploits seriality by repeatedly attacking a problem. As Lindblom pointed out, "One can easily imagine a decision maker who can return in later policy steps to no more than a few of a variety of neglected adverse consequences resulting from an earlier policy step. This possibility points directly to the need for a multiplicity of decision makers and, more than that, to a multiplicity marked by great variety of attitudes and interests, so that no line of adverse consequence fails to come to the attention of some decision maker" (1965, 151).

FIGURE 4

The Relative Superiority of Different Search Strategies



Note: α parameterizes how much the noise in evaluation is reduced by searching incrementally; β parameterizes how much riskier are distributions of new options that are generated by nonincremental search

I shall investigate the effects of a "multiplicity marked by great variety of attitudes and interests" shortly. Here I shall examine the consequences of sheer size—increasing the number of (independent) decision makers.[15]

Let there be $N \geq 3$ decision makers. The group decides by majority rule, so let N be odd. I shall assume that the decision makers are clones of each other: in every period, given any pair of alternatives, every agent has the same probability of being correct. Because adding decision makers is pointless if individual judgment is perfect, we assume that $p_{i,j;t}$ is always less than one for $i < j$ and always exceeds zero for $i > j$. As Condorcet assumed in the static setting of a single choice, the decision makers' judgments are independent (but see Ladha 1992). Judgment is insensitive to the status quo. Finally, each agent is strongly competent. We will refer to these assumptions collectively as *Condorcet conditions*.[16]

We are now ready to examine how increasing group size affects the quality of the group's choices in every period. Theorem 6 builds on the classical Condorcet result that in a static setting involving a pair of alternatives, the probability that a majority of the group will make a correct decision increases as more (equally competent and independent) decision makers are added. Though the Condorcet conditions are required, in other respects the result holds under rather general conditions. In particular, neither alternative generation nor judgment has to be stationary; they may change over time, reflecting learning or forgetting.

THEOREM 6. *Suppose the Condorcet conditions hold and both judgment and alternative generation are homogeneous. Then increases in* N *make* V_t *stochastically better, for all* $t > 0$.

To see the family resemblance between this result and Condorcet's Theorem, focus attention on the best possible policy of any given period. (Because only finitely many new options are possible in every period, for any *fixed* period a best possible policy exists, even though over time the policy space is unbounded.) Then theorem 6 implies that adding decision makers increases the chance of picking, in any fixed period, the best possible policy of that date. Similarly, adding agents decreases the chance of picking the worst possible alternative in any given period.

Interactions between Increasing Size and Seriality

It is clear that judgmental accuracy and the quality of new proposals are substitutes. The better the former, the poorer the latter can be in order to achieve a fixed level of improvement in $E[V_t]$. The opposite relation holds as well: if the decision makers have very good judgment, then the new alternatives can generally be inferior, yet the average status quo policy will still improve. And since we know that under the Condorcet conditions, increasing the group's size improves its collective competence, we should be able to offset weak policy generation by making the committee "sufficiently" large.

This intuition underlies the next result. I have assumed that every distribution of new alternatives has potential in that there is always a chance that something better than the status quo will be produced. Having potential is a very mild condition. (Indeed, it is so weak that it is obviously a *necessary* condition for $E[V_t]$ to rise: if it does not hold, then even flawless evaluation can do no more than to cling to the status quo.) Weak as it is, it provides the basis for theorem 7.

THEOREM 7. *Suppose the Condorcet conditions hold. Both judgment and alternative generation are homogeneous and stationary. Then there exists an* N^* *such that for all* $N > N^*$, $E[V_t]$ *is monotonically increasing over time.*

Given the Condorcet conditions, a sufficiently large group ensures that even a feeble alternative generation process provides enough opportunities for improvement, on average. The result holds because, under the classical assumptions, the probability that a majority of the group will vote correctly goes monotonically to one as the group grows larger. Then, to see immediately why the theorem is valid, consider the limiting case of an infallible majority. In this circumstance the committee never mistakenly discards a status quo for something inferior, and it always throws out the status quo when something better turns up. Since Q_t has potential, there is always some chance that something better than the status quo will be generated. Thus, in the limit, the average policy must improve. Since the convergence to majoritarian infallibility is monotonic, a sufficiently big N ensures that we can get as close to this ideal situation as is desired.

Naturally, the better the set of new policies, the smaller is the critical value of N^*. Consider the important special case of a martingale process: $E[Q_{t+1}|V_t = v] = v$ (March and Olsen 1984). Then, we know from theorem 2 that $N^* = 0$: if the committee is composed of only one person, $E[V_t]$ will rise steadily.

Of course, there are limits to what even a very large group can do, if new alternatives are usually very poor. Thus one can easily imagine a bad "dual" to theorem 7: for any fixed N, no matter how large, and for any fixed individual competency level, no matter how close to 1, there is a sufficiently bad distribution, Q^*_{t+1}, such that for all distributions stochastically worse than Q^*_{t+1}, policies on average degrade over time.

Interactions between Increasing Size and Incremental Search

Let us briefly return to the context of theorems 3 and 5, in which search affects only the riskiness of the distribution of new alternatives; evaluation is unaffected. Recall from theorem 5 that in policy domains where evaluation is good, the relative value of nonincremental search, $E[V'_t] - E[V_t]$, is higher than in those where evaluation is poor. We also know that under the Condorcet conditions, increasing N boosts majoritarian competence. Suppose that the Condorcet conditions hold, together with the other assumptions of theorem 5. It then follows as an immediate corollary that the relative benefits of nonlocal search are increasing in N.

What is the effect of increasing N if nonlocal search increases *both* kinds of uncertainty, by making the set of new options riskier and evaluation noisier? Suppose the incremental process (V_t) and the nonincremental process (V'_t) have the same number of decision makers. It turns out that as the groups grow larger at the same rate, the scales must eventually tip in favor of radical search.

THEOREM 8. *Suppose bolder search produces both riskier sets of new options and noisier evaluation. In each process, the Condorcet conditions hold; judgment and alternative generation are homogeneous and stationary in each. Then there exists an* $N^{\dagger}$ *such that for all* $N > N^{\dagger}$, $E[V'_t] > E[V_t]$, *for all* $t > 0$.

This result holds because of Condorcet's Theorem. His result tells us that in both incremental and nonincremental processes, majoritarian judgment gets arbitrarily close to infallibility as N rises. This implies that any difference in the evaluative abilities of the two groups must get arbitrarily small, thus allowing the other effect of nonlocal search—the greater dispersion of new alternatives—to predominate. Hence, if one thoroughly applies Lindblom's prescription of redundancy, one should *not* follow his recommendation about incremental search.

It is instructive to compare theorem 8 to Figure 4, which illustrated the fact that given a single agent, the parametric space was divided into two regions: in one, incrementalism is superior; in the other, nonincremental search.[17] In the world of Figure 4, if β is high and α is low, then $N^{\dagger} = 0$. Clearly the value of $N^{\dagger}$ must rise when we move into the region where α is high and β is low, for there local search is better than nonlocal when there is only one agent. Nevertheless, even in this region radical search is better for sufficiently big N.

MANY DECISION MAKERS, DIFFERENT GOALS

Here the agents are specialists, trained to worry about either the environmental impacts or the economic consequences of a policy, but not both. Thus, we continue to have N (odd) decision makers who use majority rule, but now they represent N different policy dimensions. An alternative is a point in this N-dimensional space. For example, if there are three dimensions, the status quo might be (1, −4, 5), and a new option might be (3, 1, −88). If the Condorcet conditions hold, then the group is more likely to pick the new option (even though it is dreadful on dimension 3) than to keep the status quo. The motivational interpretation of this behavior is that each decision maker cares only about her speciality; the cognitive interpretation, consistent with bounded rationality, is that each one *perceives* an alternative only in terms of its effect on the relevant speciality. Thus, when presented with a choice between (1, −4, 2) and (3, 1, −88), the specialist on dimension 1 sees only a choice between an option worth 1 and another worth 3.

This neglect of side effects is a major part of what Braybrooke and Lindblom call "disjointed" incrementalism.[18] In *The Intelligence of Democracy*, Lindblom calls this behavior *parametric* adjustment, noting that it is the simplest kind of partisan mutual adjustment. "It is adjustment by a decision maker who does not try to control another decision maker, does not defer to the other decision maker, does not allow the possible reactions of the other decision maker to his decision to influence the decision he makes. Whatever adjustment he achieves is due to his accommodating himself to a state of affairs without contemplating what he might do to the other decision maker" (1965, 36).

Because specialists myopically ignore the effects of an alternative on other policy areas, one might guess that this disjointed collective behavior creates systematic problems, just as individual-level heuristics cause systematic biases (see Kahneman, Slovic, and Tversky 1982). In two of the most serious problems, disjointed incrementalism can cause (1) a majority of departments to recommend a series of proposals that will on average hurt each department or (2) a majority to reject a series of proposals that would on average help every department. Both problems are illustrated by the following: given a normalized status quo of (0, 0, 0), six new alternatives are generated with equal probability: (−5, 1, 1), (1, −5, 1), (1, 1, −5) and (5, −1, −1), (−1, 5, −1), (−1, −1, 5).[19] For simplicity, assume initially that the agents evaluate flawlessly. Then the new options of (−5, 1, 1), (1, −5, 1), and (1, 1, −5) will be accepted, because in each case, two specialists correctly believe that the new alternative improves their domain. But over time this behavior will hurt everyone: the expected value of these three alternatives, on each dimension, equals $\frac{1}{6}(-5) + \frac{1}{6}(1) + \frac{1}{6}(1) = -\frac{1}{2}$. On the other hand, the options of (5, −1, −1), (−1, 5, −1), and (−1, −1, 5) will be rejected, because a majority of the agents correctly believes that the new alternative hurts their domain. But if this is done serially, they will forgo improvement in all three specialities: the expected value of these three alternatives, on each dimension, equals $\frac{1}{6}(5) + \frac{1}{6}(-1) + \frac{1}{6}(-1) = \frac{1}{2}$. Thus, although the average new alternative is just as good as the status quo, over time the average status quo will degrade. Because it is the worst options that gain majority approval, seriality will hurt rather than help. This example generalizes to a class of choice situations (theorem 9, part 2).

Of course, decision makers are fallible, so the degradation will be probabilistic rather than certain. This matters. In the example just given, infallible specialists always make the wrong collective choice; fallible ones will sometimes make the right collective choice by mistake. Thus, for this kind of problem, if evaluation becomes more accurate, the average status quo degrades *faster*, so that increasing the number of independent specialists on each dimension only makes matters worse (theorem 10, part 2).

Both of these results are obtained by generalizing the feature of the example of (−5, 1, 1), . . . , (−1, −1, 5) that poses difficulties for myopic, majoritarian incrementalism. Recall that in this example, when a new option was better for a majority of the departments, it was much worse for a third. Because this held symmetrically and because all new alternatives were equally likely, this meant that over that subset of three options, the new options were on average inferior to the status quo. Conversely, when a new option was worse for a majority of departments, it was much better for the third one. These properties turn out to be central to all remaining results, so a precise definition is warranted.

Let R, a random variable, denote the number of policy dimensions improved by a new proposal. Then the first part of the example—options (−5, 1, 1), (1, −5, 1), and (1, 1, −5)—is generalized as follows: $E[Q(X_{i,t+1})|X_{i,t} = x_i; R_{t+1} = r] < x_i$ for $N > r > \frac{N}{2}$ (when these conditional expectations are defined), for $i = 1, \ldots, N$.[20] Similarly, the second part of the example—options (5, −1, −1), (−1, 5, −1), and (−1, −1, 5)—is generalized as follows: $E[Q(X_{i,t+1})|X_{i,t} = x_i; R_{t+1} = r] > x_i$ for $0 < r < \frac{N}{2}$ (when these conditional expectations are defined), for $i = 1, \ldots, N$.[21] When both properties hold for dimension *i*, then on average progress on that dimension is inconsistent with majoritarian tendencies. Hence we will say that *i*'s new options are *majority-inconsistent*. This property describes a class of situations for which Lindblom's conjectures are invalid.

Normatively, what these situations cry out for is greater strategic sophistication. The agents should logroll across dimensions by agreeing to reject alternatives that appear to hurt one domain badly and to accept new options that they believe are a big improvement in one dimension. It is unclear, however, whether such sophistication is consistent with disjointed incrementalism.[22]

Theorem 9, part 1, and theorem 10, part 1, identify conditions ensuring that these troublesome choice situations cannot arise. Consequently, both seriality and redundancy improve matters, as Lindblom conjectured. These conditions are simply the opposite of majority inconsistency: $E[Q(X_{i,t+1})|(X_{i,t} = x_1, \ldots, X_{i,t} = x_i, \ldots, X_{N,t} = x_N); R_{t+1} = r] < x_i$ for $r < \frac{N}{2}$ and $E[Q(X_{i,t+1})|(X_{1,t} = x_1, \ldots, X_{i,t} = x_i, \ldots, X_{N,t} = x_N); R_{t+1} = r] > x_i$ for $r > \frac{N}{2}$ (when these conditional expectations are defined). When these conditions obtain for a policy dimension, expected progress in that domain is consistent with majoritarian tendencies; in short, that dimension's new alternatives are *majority-consistent*.

It is worth noting that this condition does not imply that the policy dimensions are independent.[23] Interdependence across policy domains is an important attribute that adds realism to the model. Without it, we would in effect be returned to a unidimensional policy space; the realism added via multidimensionality would be wiped out by an assumption of independence.[24]

Like theorem 2, which it resembles, theorem 9, part 1, also assumes that the expected value of new options, conditioned on a given status quo, is at least as good as that status quo's value on each dimension.

THEOREM 9. *Suppose the agents are weakly and equally competent, with crude and independent judgment.*

1. *If on every dimension* $E[Q(X_{i,t+1})|(X_{1,t} = x_1, \ldots, X_{i,t} = x_i, \ldots, X_{N,t} = x_N)] \geq x_i$ *and every dimension's new options are majority-consistent, then* $E[X_{i,t}]$ *increases monotonically over time, for all* $i = 1, \ldots, N$.
2. *If on every dimension* $E[Q(X_{i,t+1})|(X_{1,t} = x_1, \ldots, X_{i,t} = x_i, \ldots, X_{N,t} = x_N)] \leq x_i$ *and every dimension's new options are majority inconsistent, then* $E[X_{i,t}]$ *decreases monotonically over time, for all* $i = 1, \ldots, N$.

A close reading of the theorem reveals that parts 1 and 2 share an important special case, in which new proposals on average exactly equal the status quo. In this martingale process, everything depends on the pattern of conflict among policy dimensions. If new policies are majority consistent, all is well. But if they are majority inconsistent, then the expected status quo decays. Note that this difference between parts 1 and 2 does *not* depend on any difference in the decision makers' competence: in each case they are weakly competent. Even if the agents in part 2 were *more* competent than those in part 1—indeed, even if they were infallible—the average status quo policy would still degrade. Bounded rationality does matter in part 2, but it does not take the form of judgmental incompetence. What drives part 2 is myopia—inattention to an alternative's effects on a different policy domain—combined with a particular pattern of conflict across domains.

Next we reconsider the effect of increasing the number of agents. We do so symmetrically: the organization increases from N to nN, where there are $n \geq 3$ (odd) decision makers specializing in each of the N policy dimensions.[25] Here the specialists on a particular policy are considered to constitute meaningful departments: the status quo is kept if a majority of the departments vote to do so. In turn, a department will cast a vote to retain the status quo if a majority of its members vote to do so. Hence the probability that the status quo is kept is the chance that a majority of people in a majority of departments are in favor of so doing.[26]

THEOREM 10. *Suppose there are* N *departments, each with* n *agents. The Condorcet conditions hold. Judgment is crude and stationary; alternative generation is homogeneous and stationary.*

1. *If every dimension's new options are majority consistent, then* $E[X_{i,t}]$ *is monotonically increasing in* n, *for all* $i = 1, \ldots, N$ *and all* $t > 0$.
2. *If every dimension's new options are majority inconsistent, then* $E[X_{i,t}]$ *is monotonically decreasing in* n, *for all* $i = 1, \ldots, N$ *and all* $t > 0$.

Theorem 10, part 1, says that if there is the right pattern of conflict among policy areas, then adding decision makers is benign. The following example gives the intuition. Suppose the six equally likely new options are (6, 6, −1), (6, −1, 6), (−1, 6, 6) and (−6, −6, 1), (−6, 1, −6), (1, −6, −6). Then over the subset of alternatives that improve a majority of dimensions, each department is on average better off; conversely, over the subset that impairs a majority, each department is on average worse off. In such a situation one would like each department to judge as competently as possible, and under the Condorcet conditions making each unit bigger does the trick. The intuition behind part 2 is the preceding example of (−5, 1, 1), . . . , (−1, −1, 5).

Finally, we reconsider the effect of making search more or less incremental. For a single decision maker, theorem 3 showed that if search affected only the riskiness of new proposals, making search bolder improved the average status quo policy; theorem 4 showed that if search affected only the noisiness of evaluation, then incremental search was superior. Proposition 3 shows that both of these effects stand up in the multiperson setting, given majority consistency; as usual, majority inconsistency reverses the results.

Analyzing how search affects multiple decision makers who attend to different goals is complicated. To keep the problem tractable, two simplifying assumptions are used. The first simplification concerns the circumstance in which search affects only the riskiness of options (proposition 3, part 1). It is assumed that bolder search has a constant effect in each dimension *i*, by multiplying the distance from the status quo to each new option by an integer $c_i > 1$ on each dimension. For example, suppose that given a status quo of (0, . . . , 0), the more incremental process generates a new proposal of (x_1, x_2, x_3) with probability one-fourth. Then the less incremental process generates an option of (c_1x_1, c_2x_2, c_3x_3), also with probability one-fourth. (The c_i can vary across dimensions.) Since each c_i exceeds one, $Q_t'(X_i)$, the distribution of values on the i^{th} dimension produced by more radical search, is riskier than $Q_t(X_i)$, produced by more incremental search. We will call Q_t' an *expansion* of Q_t. To ensure that Q_t' is a mean-preserving spread of Q_t, it is assumed that new policies are martingales: $E[Q(X_{i,t+1})|X_{i,t} = x_i] = x_i$.

The second simplification pertains to how search affects the evaluation of new options (proposition 3, part 2). The process of judgment is black-boxed, and it is simply maintained that less incremental search makes evaluation less reliable: $p_{i,j}' < p_{i,j}$ for $i < j$ and $p_{i,h}' > p_{i,h}$ for $i > h$.

PROPOSITION 3. *The decision makers are weakly and equally competent; judgments are independent and crude. Alternative generation is homogeneous.*

1. *Suppose bolder search yields riskier distributions of options. New policies are martingales, and* Q_t' *is an expansion of* Q_t. *If every dimension's new options are majority-consistent, then* $E[X_{i,t}'] > E[X_{i,t}]$ *for all* $i = 1, \ldots, N$ *and all* $t > 0$; *if they are majority-inconsistent, then* $E[X_{i,t}'] < E[X_{i,t}]$ *for all* $i = 1, \ldots, N$ *and all* $t > 0$.
2. *Suppose bolder search produces less reliable evaluation. If every dimension's new options are majority-consistent, then* $E[X_{i,t}'] < E[X_{i,t}]$ *for all* $i = 1, \ldots, N$ *and all* $t > 0$; *if they are majority-inconsistent, then* $E[X_{i,t}'] > E[X_{i,t}]$ *for all* $i = 1, \ldots, N$ *and all* $t > 0$.

The key to understanding proposition 3 is realizing that it satisfies the conditions of theorem 9. Thus, in proposition 3, part 1, if majority consistency holds, theorem 9 implies that regardless of the degree of incrementalism, the expected status quo improves. Since the average new policy just equals the status quo, credit for improvement must go to the process of collective evaluation, which weights the good part of the set of new options (where the average new option exceeds the status quo) more than the bad part. Given this, the benefit of scaling up the distribution's good part by a positive constant must outweigh the cost of scaling up its bad part by the same amount. So it pays to search more boldly, because doing so yields expanded (scaled-up) distributions. When majority inconsistency holds, Theorem 9 says that the average status quo falls, because collective evaluation weights the distribution's bad part more than the good. Given this, scaling up the bad and good parts equally is harmful. Thus, more local search, which produces more compact distributions, is superior.

Proposition 3, part 2, is straightforward. Given majority consistency across policy domains, majoritarian tendencies are beneficial for each department. Therefore, one wants departments to be as judgmentally competent as possible. Hence if nonincremental search makes evaluation less reliable, while leaving the distribution of new options unchanged, then it must be harmful.

But search probably affects both kinds of uncertainty simultaneously. What then can be said about the *net* effect of more- versus less-incremental search? Again Condorcet comes to the rescue. Recall that theorem 8 showed that when both effects are present, bolder search is superior if the (harmonious) group is sufficiently big. This holds when there is conflict, so long as majority consistency obtains.

PROPOSITION 4. *Suppose bolder search produces both riskier distributions of new options (via expansions) and less reliable evaluation. The Condorcet conditions hold. Judgments are crude and stationary. New policies are martingales, generated by a homogeneous and stationary process. Each department has* n *agents.*

1. *If every dimension's new options are majority-consistent, then there is an* n^+ *such that for all* $n > n^+$, $E[X_{i,t}'] > E[X_{i,t}]$ *for all* $i = 1, \ldots, N$ *and all* $t > 0$.
2. *If every dimension's new options are majority-inconsistent, then there is an* n^+ *such that for all* $n > n^+$, $E[X_{i,t}'] < E[X_{i,t}]$ *for all* $i = 1, \ldots, N$ *and all* $t > 0$.

Proposition 4, part 1 suggests that there is nothing inherent about conflict that makes nonincremental search less valuable. What does cause problems for bold search is (as part 2 reveals) a particular *pattern* of conflict—majority inconsistency. When this pattern holds, the more localized search is, the better.

But part 2 gives little comfort to Lindblom's theory, for the property that makes incremental search superior yielded the *wrong* effect regarding the theory's uncontroversial claims about seriality and redundant agents. When new alternatives were majority-inconsistent, theorem 10 reported that adding decision makers *worsened* the average status quo policy and theorem 9 reported that making choices repeatedly did likewise. Hence the informal theory of disjointed incrementalism faces a dilemma: conditions confirming its uncontroversial claims help to disconfirm its claims about the benefits of local search; and conversely, conditions upholding the conjectures about incremental search yield unexpected results about seriality and redundancy.

CONCLUSION

The results show that when there is a single decision maker or a harmonious group, then working on a problem repeatedly or adding new agents are helpful heuristics, as Lindblom hypothesized. But when there are many decision makers with conflicting goals, even these procedures can backfire: Lindblom's claims, widely regarded as uncontroversial, turn out to be invalid under the not-implausible circumstance of majority inconsistency.

These results underscore the significance of the relation between *heuristics* and the *biases* they can produce. This line of research, pioneered by Kahneman and Tversky in the context of single-person judgment and choice, has now been extended by Neale and Bazerman (1991) to multiperson strategic settings. Given the recency of these developments, it is unsurprising that the much older theory of incrementalism failed to analyze how the heuristics embodied in muddling through might create systematic problems under certain conditions. But that all heuristics can do so—no matter how useful in certain environments—is now conventional wisdom in behavioral decision theory. Applying this insight to the study of complex political institutions—in particular, understanding how heuristics adapted to certain patterns of conflict are maladapted to others—should be an important item on our research agenda.

Consider now the purported benefits of incrementalism's most hotly debated heuristic, local search. Here it turns out that the informal theory runs into problems even in the context of a *single* decision maker. Even in this simple setting, the advantages of incrementalism are equivocal. One reason for this lack of clarity is that the original literature did not specify precisely how search affected uncertainty. Thus some conceptual unpacking was required. It was suggested that Lindblom and his critics believed that nonincremental search created two very different kinds of uncertainty: a riskier distribution of new alternatives and noisier estimates of the value of any given new alternative. These two kinds of uncertainty affected the average status quo policy in sharply contrasting ways. Theorem 3 and proposition 2 showed that greater uncertainty concerning the distribution of new alternatives is beneficial, whereas theorem 4 and proposition 1 showed that noisier evaluations are harmful.

These results are analytical: they examine one type of uncertainty while holding the other constant. The problem is that as one searches less locally, both kinds of uncertainty will probably increase, and the *net* effect of these two changes is uncertain. However, theorem 8 tells us that the net effect will favor bold search if there are enough decision makers who, Condorcet-fashion, produce collectively reliable evaluations. And proposition 4, part 1, tells us that under the same condition (majority consistency) that confirmed the verbal theory's uncontroversial claims, the net effect still favors radical search. Thus the logic of incrementalism's benefits is either unclear or rejected. Because this is one of my more surprising results, it merits a few concluding remarks.

First, in some policy areas improving on current solutions may be hard because most of the promising ideas have been tried (Kingdon 1984). Or perhaps the cost of searching rises sharply as policy design becomes more innovative. For either reason, nonincremental search might generate new options with lower expected net value than those produced by local search. (Contrast this with the assumption used here of riskier distributions being *mean-preserving* spreads of safer ones.) To be sure, simply asserting that nonincremental search produces poorer average new alternatives is not a deep point theoretically: as stated, it is either a theoretical assumption (not a derivation) or an empirical generalization.[27] Nevertheless, if true it would certainly be of great pragmatic importance.

Second, a related empirical claim is that *implementing* radically new programs is harder than implementing marginally different ones (Braybrooke 1985). In addition to case study evidence (e.g., Derthick 1990), the phenomenon of the "learning curve"—the tendency of firms to reduce costs as they gain experience in manufacturing a new product (Argote and Epple 1990)—provides systematic quantitative support for this proposition.

Third, the models in this paper, by focusing on the quality of policies rather than on agents' subjective preferences, implicitly presumed risk neutrality. But if decision makers are risk-averse, they may prefer incremental search simply because it reduces uncertainty by generating less dispersed distributions of options. However, sweeping assertions about the effect of risk aversion may be unfounded. Though it is tautologically true that a risk-averse agent prefers, for example, a sure thing to a lottery with the same expected value, this is *not* the choice that confronts decision makers in status-quo-based policy processes. In these contexts, one can reject the new proposal and keep the status quo policy in force. By providing a lower bound on what the decision makers will get, the status quo truncates the distribution of new options by chopping off part of the "bad tail." This differs significantly from the choice between a sure thing and a lottery: if one chooses the latter, there is no lower bound other than the lottery's minimal value, leaving one fully exposed to downside risk. Thus introducing risk aversion would not yield a conclusive answer favoring more incremental search. Risk-averse decision makers in status-quo-based policy processes would have to trade-off reducing a bad (more dispersion) versus increasing a good (up-side risk).

Finally, in translating an informal theory into a formal model one always runs the risk of failing to represent the former correctly. Clearly, one property of Lindblom's informal theory is not captured by the models here—the trade-off between many frequent small steps and few infrequent big ones (Lindblom 1959, 86). This is a plausible trade-off: even an institution accustomed to parallel processing, such as Congress, cannot pass many *major* bills in the same session. Major legislation requires a scarce resource—the attention of leaders. One could partly capture this by assuming that incremental search could occur every period but nonincremental search only every s periods. Obviously, if s is "sufficiently" large, then incrementalism is better.[28]

In any case, the models presented here are only a first step in clarifying the logic and claims of incrementalism. By the nature of formal models, they make clear which aspects of the complex strategy of muddling through have been represented and which omitted. This should make further steps (large or small) easier.

APPENDIX

Theorem 1: *If the decision maker is weakly competent, judgment is crude and stationary, and the distribution of new proposals is independent of the status quo, then* V_t *stochastically improves with time.*

Proof. Recall that the new alternatives are independently and identically distributed. Define $\gamma \equiv \Sigma_{k=i}^{M} q_k$, that is, the "good tail" of the new options. Then for $i = 2, \ldots, M$,

$$p(V_{t+1} \geq i) = p(V_t < i) \cdot \gamma \cdot f$$
$$+ p(V_t \geq i)[(1 - \gamma)(1 - b) + \gamma]$$

Let $\beta \equiv f\gamma$ and $\alpha \equiv (1 - \gamma)(1 - b) + \gamma = 1 - b + b\gamma$. Then

$$\begin{aligned} p(V_{t+1} \geq i) &= \beta \cdot p(V_t < i) + \alpha \cdot p(V_t \geq i) \\ &= \beta[1 - p(V_t \geq i)] + \alpha \cdot p(V_t \geq i) \\ &= \beta + (\alpha - \beta) \cdot p(V_t \geq i). \end{aligned}$$

Mutatis mutandis,

$$p(V_t \geq i) = \beta + (\alpha - \beta) \cdot p(V_{t-1} \geq i).$$

Substituting,

$$\begin{aligned} p(V_{t+1} \geq i) &= \beta + (\alpha - \beta)[\beta + (\alpha - \beta) \cdot p(V_{t-1} \geq i)] \\ &= \beta + \beta(\alpha - \beta) + (\alpha - \beta)^2 \cdot p(V_{t-1} \geq i). \end{aligned}$$

And in general, for any k, where $0 \leq k < t$,

$$p(V_t \geq i) = \beta + \beta(\alpha - \beta) + \cdots + \beta(\alpha - \beta)^{t-k-1}$$
$$+ (\alpha - \beta)^{t-k} \cdot p(V_k \geq v_i).$$

So, specifically,

$$p(V_{t+1} \geq i) = \beta + \beta(\alpha - \beta) + \cdots + \beta(\alpha - \beta)^t$$
$$+ (\alpha - \beta)^{t+1} \cdot p(V_0 \geq v_i),$$

where

$$p(V_t \geq i) = \beta + \beta(\alpha - \beta) + \cdots + \beta(\alpha - \beta)^{t-1}$$
$$+ (\alpha - \beta)^t \cdot p(V_0 \geq v_i).$$

Thus, after canceling, $p(V_{t+1} \geq i) > p(V_t \geq i)$ iff

$$\beta(\alpha - \beta)^t + (\alpha - \beta)^{t+1} \cdot p(V_0 \geq v_i)$$
$$> (\alpha - \beta)^t \cdot p(V_0 \geq v_i) \quad \textbf{(1)}$$

Thus establishing inequality (1) will complete the proof. Since $\gamma \geq f\gamma \rightarrow \alpha > \beta$, we can cancel $(\alpha - \beta)^t$ from both sides, reducing relationship (1) to

$$\beta + (\alpha - \beta) \cdot p(V_0 \geq v_i) > p(V_0 \geq v_i)$$
$$\beta > [1 - (\alpha - \beta)] \cdot p(V_0 \geq v_i)$$

or, since $p(V_0 \geq v_i) = \gamma$, $\beta > [1 - (\alpha - \beta)]\gamma$.

To demonstrate that $\beta > [1 - (\alpha - \beta)]\gamma$, recall that $f > b$. Working backward from this,

$$\begin{aligned} f\gamma(1 - \gamma) &> b\gamma(1 - \gamma) = \gamma(b - b\gamma) \\ \beta(1 - \gamma) &> \gamma(b - b\gamma) = \gamma(1 - \alpha) \\ \beta &> \gamma(1 - \alpha) + \beta\gamma = \gamma(1 - \alpha + \beta). \end{aligned}$$

Q.E.D.

Theorem 2: *If* $E[Q_{t+1}|V_t = i] \geq i$ *and the decision maker is weakly competent, then* $E[V_t]$ *increases monotonically over time.*

Proof. Define $\min\{V_t\} \equiv M_{1,t}$ and $\max\{V_t\} \equiv M_{2,t}$. Then

$$E[V_{t+1}] = \sum_{i=M_{1,t}}^{M_{2,t}} E[V_{t+1}|V_t = i] \cdot p(V_t = i).$$

Obviously, if $E[V_{t+1}|V_t = i] > i$ for every value of V_t, then $E[V_{t+1}] > E[V_t]$. Hence we can focus our attention on showing that $E[V_{t+1}|V_t = i] > i$ for an arbitrary i.

Fix a status quo i. To compute a lower bound of $E[V_{t+1}]$, assume that for all new alternatives j, $p_{i,j;t+1}$ equals some constant p, where $p_{i,i-1;t+1} < p < p_{i,i+1;t+1}$. Since weak competency requires that $p_{i,i-1;t+1} < p_{i,i+1;t+1}$, this lower bound must be strictly less than $E[V_{t+1}]$, since that expectation is monotonically increasing in every $p_{i,j;t+1}$.

For any fixed status quo i, define $i - m_{1,t+1} \equiv \min\{Q_{t+1}|V_t = i\}$ and $i + m_{2,t+1} \equiv \max\{Q_{t+1}|V_t = i\}$. As a notational convenience, we will suppress the time subscript on these minimum and maximum values, as well as on the $q_{i,j}$'s, understanding that they are all a function of $t + 1$. Then we get as a lower bound

$$\begin{aligned} E[V_{t+1}|V_t = i] \\ &= p \cdot \left(\sum_{j=i-m_1}^{i-1} q_{i,j} + \sum_{k=i+1}^{i+m_2} q_{i,k} \right) \\ &\quad + (q_{i,i} + (1 - p)(1 - q_{i,i}))i \\ &= p \cdot \left(\sum_{j=i-m_1}^{i-1} q_{i,j} + \sum_{k=i+1}^{i+m_2} q_{i,k} \right) \\ &\quad + (pq_{i,i})i + ((1 - p)q_{i,i})i + (1 - p)(1 - q_{i,i})i \\ &= p \cdot E[Q_{t+1}|V_t = i] + (1 - p)i \geq i. \end{aligned}$$

Q.E.D.

Theorem 3: *Suppose search in the V process is more incremental than in the V′ process, in every period. The* *decision maker is weakly competent, with crude judgment. Alternative generation in each process is homogeneous. Then* $E[V'_t] > E[V_t]$, *for all* $t > 0$.

Proof. The following lemma is very useful in proving theorem 3 and several other results. (The proofs of lemma 1 and of the other lemmas used in this Appendix are available on request from the author.)

LEMMA 1. *Suppose judgment and alternative generation are homogeneous. Then to prove that* $E[V'_t] > E[V_t]$, *it suffices to show that* $E[V'_{t+1}|V'_t = 0] > E[V_{t+1}|V_t = 0]$, *for an arbitrary date* $t \geq 1$.

Given lemma 1, it suffices to show that $E[V'_t|V'_{t-1} = 0] > E[V_t|V_{t-1} = 0]$. Define $\min\{Q_t|V_{t-1} = i\} \equiv i - m_{1,t}$ and $\max\{Q_t|V_{t-1} = i\} \equiv i + m_{2,t}$. Parallel meanings apply to $i - m'_{1,t}$ and $i + m'_{2,t}$. Then

$$\begin{aligned} E[V_t|V_{t-1} = 0] &= b_t\left(\sum_{j=-m_{1,t}}^{-1} q_{0,j;t} \cdot j\right) + f_t\left(\sum_{k=0}^{m_{2,t}} q_{0,k;t} \cdot k\right) \\ &= b_t\left(\sum_{j=-m_{1,t}}^{-1} q_{0,j;t} \cdot j\right) + b_t\left(\sum_{k=0}^{m_{2,t}} q_{0,k;t} \cdot k\right) \\ &\quad + (f_t - b_t)\left(\sum_{k=0}^{m_{2,t}} q_{0,k;t} \cdot k\right) \\ &= b_t(E[Q_t|V_{t-1} = 0]) + (f_t - b_t)\left(\sum_{k=0}^{m_{2,t}} q_{0,k;t} \cdot k\right). \end{aligned}$$

Mutatis mutandis,

$$E[V'_t|V'_{t-1} = 0] = b_t(E[Q'_t|V'_{t-1} = 0]) + (f_t - b_t)\left(\sum_{k=0}^{m'_{2,t}} q'_{0,k;t} \cdot k\right).$$

Since Q'_t is a mean-preserving spread of Q_t,

$$E[Q_t|V_{t-1} = 0] = E[Q'_t|V'_{t-1} = 0], \text{ so } E[V_t|V_{t-1} = 0] \gtrless E[V'_t|V'_{t-1} = 0]$$

just as

$$\sum_{k=0}^{m_{2,t}} q_{0,k;t} \cdot k \gtrless \sum_{k=0}^{m'_{2,t}} q'_{0,k;t} \cdot k.$$

Because Q'_t is a mean-preserving spread of Q_t, it follows that $m'_{2,t} \geq m_{2,t}$. Then, rewriting, we get

$$\begin{aligned} \sum_{k=0}^{m_{2,t}} q_{0,k;t} \cdot k = q_{0,1;t} + q_{0,2;t} + \cdots &+ q_{0,m'_{2,t};t} \\ + q_{0,2;t} + \cdots &+ q_{0,m'_{2,t};t} \\ + \cdots& \\ &+ q_{0,m'_{2,t};t}. \end{aligned}$$

Similarly,

$$\begin{aligned} \sum_{k=0}^{m'_{2,t}} q'_{0,k;t} \cdot k = q'_{0,1;t} + q'_{0,2;t} + \cdots &+ q'_{0,m'_{2,t};t} \\ + q'_{0,2;t} + \cdots &+ q'_{0,m'_{2,t};t} \\ + \cdots& \\ &+ q'_{0,m'_{2,t};t}. \end{aligned}$$

Since Q'_t is riskier than Q_t, $p(Q'_t > k|V'_{t-1} = 0) \geq p(Q_t > k|V_{t-1} = 0)$ for all $k > 0$. That is, $q'_{0,k;t} + \cdots + q'_{0,m'_{2,t};t} \geq q_{0,k;t} + \cdots + q_{0,m'_{2,t};t}$ for $k = 1, \ldots, m'_{2,t}$. Since this inequality must be strict for at least one $k > 0$, it follows that $E[V'_t|V'_{t-1} = 0] > E[V_t|V_{t-1} = 0]$. *Q.E.D.*

Theorem 4: *Suppose search in the* V'_t *process is less incremental than in the* V_t *process. Less incremental search produces noisier evaluation. Alternative generation in the two processes is identical, and is homogeneous. Then* V_t *is (weakly) stochastically better than* V'_t, *for all* $t > 0$.

Proof. The proof is by induction: it will be shown that if V_t is weakly stochastically better than V'_t, then V_{t+1} will be weakly stochastically better than V'_{t+1}.

The proof has three parts. Part (a) shows that the assumptions about noisy evaluation imply that for all $i < j$, $p_{i,j;t} \geq p'_{i,j;t}$ and for all $i > j$, $p_{i,j;t} \leq p'_{i,j;t}$ (where $p_{i,j;t}$ and $p'_{i,j;t}$ are associated with the V_t and the V'_t process, respectively). Parts (b) and (c) show that this implies that V_{t+1} is weakly stochastically better than V'_{t+1}.

PART (a). Consider the probability of selecting a new option over the status quo in period t in each process. First focus on the case where $i < j$ so that $p_{i,j;t}$ denotes the chance of correctly selecting a superior new option over the status quo i.

Recall that $\tilde{Q}_t = Q_t + \theta_t$ and $\tilde{Q}'_t = Q'_t + \theta'_t$. Given this structure of error, $1 - p_{i,j;t} = p(j + \theta_t \leq i)$ and $1 - p'_{i,j;t} = p(j + \theta'_t \leq i)$. By assumption, θ'_t is noisier (riskier) than θ_t, which by definition means that $p(\theta'_t < x) \geq p(\theta_t < x)$ for all $x < 0$. Hence, in particular, $p(\theta'_t < i - j) \geq p(\theta_t < i - j)$. This in turn implies that $p(j + \theta'_t < i) \geq p(j + \theta_t < i)$, whence $p'_{i,j;t} \leq p_{i,j;t}$. The same reasoning shows that $p'_{i,j;t} \geq p_{i,j;t}$ whenever $i > j$.

PART (b). As an intermediate step, compare the less incremental process, V'_t, with a third process, denoted by W_t. At date t, W_t is distributed identically to V'_t—hence both are weakly stochastically worse than V_t—but the $p_{i,j;t}$'s of W_t are identical to those of the V_t process.

Define $\min\{V_t\} \equiv M_{1,t}$ and $\max\{V_t\} \equiv M_{2,t}$. Because the generation probabilities are the same, V_t, V'_t, and W_t have the same support, hence the same minimum and maximum values in every period. Then

$$p(W_{t+1} > v) = \sum_{i=M_{1,t}}^{M_{2,t}} p(W_{t+1} > v|W_t = i) \cdot p(W_t = i)$$

whereas

$$p(V'_{t+1} > v) = \sum_{i=M_{1,t}}^{M_{2,t}} p(V'_{t+1} > v|V'_t = i) \cdot p(V'_t = i).$$

Since by assumption $p(W_t = i) = p(V'_t = i)$ for all $i = M_{1,t}, \ldots, M_{2,t}$, it suffices to show that $p(W_{t+1} > v|W_t = i) \geq p(V'_{t+1} > v|V'_t = i)$ for all i. For any fixed status quo i, define $i - m_{1,t+1} \equiv \min\{Q_{t+1}|V'_t = i\}$ and $i + m_{2,t+1} = \max\{Q_{t+1}|V'_t = i\}$. Then for any fixed value of v, we can partition all possible new alternatives of period $t + 1$ into five disjoint and collectively exhaustive sets:

1. $i + m_{2,t+1} \leq v$
2. $i < v < i + m_{2,t+1}$
3. $i = v$
4. $i - m_{1,t+1} \leq v < i$
5. $v < i - m_{1,t+1}$

Obviously, for set 1, we get $p(W_{t+1} > v|W_t = i) = 0 = p(V'_{t+1} > v|V'_t = i)$, and for set 5, we get $p(W_{t+1} > v|W_t = i) = 1 = p(V'_{t+1} > v|V'_t = i)$. Thus the decisive sets are 2–4. For set 2,

$$p(V'_{t+1} > v|V'_t = i) = \sum_{j=v+1}^{i+m_{2,t+1}} q_{i,j;t+1}p'_{i,j;t+1},$$

which is less than or equal to

$$p(W_{t+1} > v|W_t = i) = \sum_{j=v+1}^{i+m_{2,t+1}} q_{i,j;t+1}p_{i,j;t+1}$$

because the $q_{i,j}$'s are identical and $p_{i,j;t+1} \geq p'_{i,j;t+1}$ for every j. The comparisons for sets 3 and 4 are analogous. Hence W_{t+1} is weakly stochastically better than V'_{t+1}.

Part (c). Now compare V_{t+1} to W_{t+1}. Comparing

$$p(W_{t+1} > v) = \sum_{i=M_{1,t}}^{M_{2,t}} p(W_{t+1} > v|W_t = i) \cdot p(W_t = i)$$

to

$$p(V_{t+1} > v) = \sum_{i=M_{1,t}}^{M_{2,t}} p(V_{t+1} > v|V_t = i) \cdot p(V_t = i),$$

we see that here the conditional probabilities are the same since the W-process and the V-process share the same $q_{i,j}$'s and the same $p_{i,j}$'s; what differs are the unconditional probabilities at date t.

Define $\alpha \equiv p(W_{t+1} > v|W_t = i)$. Because alternative generation is homogeneous across status quos ($q_{i,i+k} = q_{j,j+k}$), better status quos shift the distribution of new options "for the better," that is, to the right. And because judgment is also homogeneous across status quos, due to θ_t being constant across status quos, this shift to the right is preserved by evaluation. Hence $\alpha_i \leq \alpha_{i+1}$, for all i. Now rewrite:

$$p(W_{t+1} > v) = \sum_{i=M_{1,t}}^{M_{2,t}} \alpha_i \cdot p(W_t = i)$$

$$= \alpha_{M_{1,t}} \sum_{i=M_{i,t}}^{M_{2,t}} p(W_t = i) + (\alpha_{M_{1,t}+1} - \alpha_{M_{1,t}}) \sum_{i=M_{1,t}+1}^{M_{2,t}} p(W_t = i) + \cdot \cdot \cdot + (\alpha_{M_{2,t}} - \alpha_{M_{2,t}-1}) \sum_{i=M_{2,t}}^{M_{2,t}} p(W_t = i).$$

Similarly,

$$p(V_{t+1} > v) = \alpha_{M_{1,t}} \sum_{i=M_{1,t}}^{M_{2,t}} p(V_t = i) + (\alpha_{M_{1,t}+1} - \alpha_{M_{1,t}}) \sum_{i=M_{1,t}+1}^{M_{2,t}} p(V_t = i) + \cdot \cdot \cdot + (\alpha_{M_{2,t}} - \alpha_{M_{2,t}-1}) \sum_{i=M_{2,t}}^{M_{2,t}} p(V_t = i)$$

Because the α_i's are weakly increasing, every term $(\alpha_i - \alpha_{i-1})$ is nonnegative. And because V_t is weakly stochastically bigger than W_t,

$$\sum_{i=j}^{M_{2,t}} p(V_t = i) \geq \sum_{i=j}^{M_{2,t}} p(W_t = i)$$

$$\text{for every } j = M_{1,t} + 1, \ldots, M_{2,t}.$$

Then comparing term by term, it follows immediately that $p(V_{t+1} > v) \geq p(W_{t+1} > v)$. This, combined with the conclusion of part (a) yields the result that V_{t+1} is weakly stochastically better than V'_{t+1}, if V_t is weakly stochastically better than V'_t.

To start the induction, we must show that V_t is weakly stochastically better than V'_t for a specific t. This must hold for $t = 0$: because there is no evaluation in that period, the status quo at the end of period 0 is just a draw from the vector of initial probabilities. This vector is the same for both processes; hence V_0 and V'_0 are distributed identically. Because V_0 and V'_0 stand in the same relation to each other as do W_t and V'_t in part (a) of this proof, it follows immediately that V_1 is weakly stochastically better than V'_1. *Q.E.D.*

Theorem 5: *If the conditions of Theorem 3 hold, then the relative benefits of nonincremental search,* $E[V'_t] - E[V_t]$, *increase as* f *rises or* b *falls.*

Proof. Under the conditions of theorem 3, we know from lemma 1 that

$$E[V_t] = E[Q_0] + \sum_{s=1}^{t} \mu_s, \text{ where } \mu_s \equiv E[V_s|V_{s-1} = 0].$$

Similarly,

$$E[V'_t] = E[Q_0] + \sum_{s=1}^{t} \mu'_s, \text{ where } \mu'_s \equiv E[V'_s|V'_{s-1} = 0].$$

It therefore suffices to show that for an arbitrary s, $\mu'_s - \mu_s$ is increasing in the accuracy of policy evaluation. Now, theorem 3 gave us that

$$\mu'_s - \mu_s = b_s(E[Q'_s|V'_{s-1} = 0]) + (f_s - b_s)\left(\sum_{k=0}^{m'_{2,s}} q'_{0,k} \cdot k\right)$$

$$- \left\{ b_s(E[Q_s|V_{s-1} = 0]) + (f_s - b_s)\left(\sum_{k=0}^{m_{2,s}} q_{0,k} \cdot k\right)\right\}$$

$$= (f_s - b_s)\left\{\sum_{k=0}^{m'_{2,s}} q'_{0,k} \cdot k - \sum_{k=0}^{m_{2,s}} q_{0,k} \cdot k\right\}$$

Also from theorem 3, we know that

$$\sum_{k=0}^{m'_{2,s}} q'_{0,k} \cdot k > \sum_{k=0}^{m_{2,s}} q_{0,k} \cdot k, \text{ so } \frac{\partial(\mu'_s - \mu_s)}{\partial f_s} > 0$$

$$\text{and } \frac{\partial(\mu'_s - \mu_s)}{\partial b_s} < 0.$$

Note that the proof never used the assumption of weak competency ($f > b$). *Q.E.D.*

Theorem 6: *Suppose the Condorcet conditions hold, and both judgment and alternative generation are homogeneous. Then increases in* N *make* V_t *stochastically better, for all* $t > 0$.

Proof. Let V_t denote a process with N decision makers, V'_t, a process with $N' > N$ agents. Similarly $p_{i,j;t}$ and $p'_{i,j;t}$ denote evaluation probabilities of their respective processes. Since $N' > N$ (with both odd) and since each individual is fallible, $p'_{i,j;t} > p_{i,j;t}$. Given this, the proof of theorem 6 exactly follows the logic of parts (b) and (c) of theorem 4, except that here it is shown that V'_t is stochastically better than V_t (instead of showing that V_t is weakly stochastically better than V'_t). *Q.E.D.*

Theorem 7: *Suppose the condorcet conditions hold. Both judgment and alternative generation are homogeneous and stationary. Then there exists an* N^* *such that for all* $N > N^*$, $E[V_t]$ *is monotonically increasing over time.*

Proof. The proof will show that given the theorem's assumptions, $E[V_{t+1}] > E[V_t]$. We use the fact that in general, for two random variables X and Y, $E[Y] = E[E[Y|X = x]]$, taken over all values of X. It is obvious that if for *every* value of V_t, $E[V_{t+1}|V_t = v] > v$, then $E[V_{t+1}] > E[V_t]$. Hence it suffices to show that the conditional expectation of V_{t+1} exceeds v.

If every $p_{i,j} = 1$, mistakes never happen. Therefore $E[V_{t+1}|V_t = v]$ is a weighted average of v and those values of Q_{t+1} that exceed v. Since Q_{t+1} has potential, such values must exist, so $E[V_{t+1}|V_t = v]$ must strictly exceed v.

Because $E[V_{t+1}|V_t = v]$ is the sum of a set of terms, each of which is continuously increasing in $p_{i,j}$, $E[V_{t+1}|V_t = v]$ itself is continuously increasing in $p_{i,j}$. Thus there exists a $p^* < 1$ such that for all $p_{i,j} > p^*$, $E[V_{t+1}|V_t = v] > v$.

Under the Condorcet conditions, each $p_{i,j}$ rises monotonically toward 1 as N increases, so for sufficiently large N, each $p_{i,j} > p^*$. *Q.E.D.*

Theorem 8: *Suppose bolder search produces both riskier distributions of new alternatives and noisier evaluation. In each process, the Condorcet conditions hold; judgment and alternative generation are homogeneous and stationary in each. Then there exists an* $N^\dagger$ *such that for all* $N > N^\dagger$, $E[V'_t] > E[V_t]$, *for all* $t > 0$.

Proof. By lemma 1, it suffices to show that $E[V'_{t+1}|V'_t = 0] > E[V_{t+1}|V_t = 0]$. As usual, define $-m_{1,t+1} \equiv \min\{Q_{t+1}|V_t = 0\}$ and $m_{2,t+1} \equiv \max\{Q_{t+1}|V_t = 0\}$. Analogous meanings hold for $-m'_{1,t+1}$ and $m'_{2,t+1}$. Necessarily, $-m'_{1,t+1} \leq -m_{1,t+1} < 0 < m_{2,t+1} \leq m'_{2,t+1}$. Due to stationarity, the time subscripts can be suppressed. Then

$$E[V_{t+1}|V_t = 0] = \sum_{h=-1}^{-m_1} q_{0,h}p_{0,h}h + \sum_{j=1}^{m_2} q_{0,j}p_{0,j}j$$

$$E[V'_{t+1}|V'_t = 0] = \sum_{h=-1}^{-m'_1} q'_{0,h}p'_{0,h}h + \sum_{j=1}^{m'_2} q'_{0,j}p'_{0,j}j$$

By Condorcet's Theorem, $\lim_{N\to\infty} p_{0,h} = \lim_{N\to\infty} p'_{0,h} = 0$ and $\lim_{N\to\infty} p_{0,j} = \lim_{N\to\infty} p'_{0,j} = 1$. Hence

$$\lim_{N\to\infty} E[V_{t+1}|V_t = 0] = \sum_{j=1}^{m_2} q_{0,j}j$$

$$\lim_{N\to\infty} E[V'_{t+1}|V'_t = 0] = \sum_{j=1}^{m'_2} q'_{0,j}j.$$

Theorem 3 tells us that the expected value of the good tail of a mean-preserving riskier distribution exceeds that of a safer one, that is, $\Sigma_{j=1}^{m'_2} q'_{0,j}j > \Sigma_{j=1}^{m_2} q_{0,j}j$. And because $E[V'_{t+1}|V'_t = 0]$ converges monotonically to $\Sigma_{j=1}^{m'_2} q'_{0,j}j$, it follows that there must exist an $N^\dagger$ sufficiently large such that for all $N > N^\dagger$, $E[V'_{t+1}|V'_t = 0] > E[V_{t+1}|V_t = 0]$. *Q.E.D.*

Theorem 9: *Suppose the agents are weakly and equally competent, with crude judgment. Judgments are independent.*

1. *If on every dimension* $E[Q(X_{i,t+1})|(X_{1,t} = x_1, \ldots, X_{i,t} = x_i, \ldots, X_{N,t} = x_N)] \geq x_i$ *and every dimension's new options are majority consistent, then* $E[X_{i,t}]$ *increases monotonically over time, for all* $i = 1, \ldots, N$.
2. *If on every dimension* $E[Q(X_{i,t+1})|(X_{1,t} = x_1, \ldots, X_{i,t} = x_i, \ldots, X_{N,t} = x_N)] \leq x_i$ *and every dimension's new options are majority inconsistent, then* $E[X_{i,t}]$ *decreases monotonically over time, for all* $i = 1, \ldots, N$.

Proof. Part (a) is proven here; the proof of part (b) is symmetric and is therefore omitted.

Let $q_{(X_{1,t}=x_1, \ldots, X_{N,t}=x_N),r;t}$ denote the probability that given a status quo of $(X_{1,t} = x_1, \ldots, X_{N,t} = x_N)$, the new alternative would be an improvement in r policy dimensions. Thus this is the probability of generating a *set* of new options from a given status quo. For convenience, shorten this to $q_{sq,r;t}$.

Let $P_{sq,(X_{1,t+1}=x_1, \ldots, X_{N,t+1}=x_N;r)}$ denote the probability that the group of specialists vote to accept a new option over the status quo, and in this option r dimensions are improved relative to the status quo ($r = 0, \ldots, N$). Because the decision makers are equally competent, with crude and independent judgment, this probability is constant for a fixed r.[29] For convenience, let $P_{sq,r}$ denote this probability. Then

$$E[X_{i,t+1}|(X_{1,t} = x, \ldots, X_{N,t} = x_N)]$$

$$= \sum_{k=0}^{N} q_{sq,r;t} \cdot E[Q(X_{i,t+1})|sq;r] \cdot P_{sq,r}.$$

Without loss of generality, normalize the status quo so that $X_{i,t} = 0$. We then use the following lemma to construct a lower bound for $E[X_{i,t+1}|sq]$.

LEMMA 2. $\mathrm{P}_{\mathrm{sq,r}}$ *is strictly increasing in* r.

We thus have that

$$E[X_{i,t+1}|(X_{1,t} = x_1, \ldots, X_{N,t} = x_N)]$$

$$\geq P_{sq,\frac{N-1}{2}} \left(\sum_{r=0}^{\frac{N-1}{2}} q_{sq,r;t} \cdot E[Q(X_{i,t+1})|sq;r] \right)$$

$$+ P_{sq,\frac{N+1}{2}} \left(\sum_{r=\frac{N+1}{2}}^{N} q_{sq,r;t} \cdot E[Q(X_{i,t+1})|sq;r] \right).$$

By lemma 2, $P_{sq,(N+1)/2} > P_{sq,(N-1)/2}$; and since

$$\sum_{r=0}^{\frac{N-1}{2}} q_{sq,r;t} \cdot E[Q(X_{i,t+1})|sq;r]$$

$$+ \sum_{r=\frac{N+1}{2}}^{N} q_{sq,r;t} \cdot E[Q(X_{i,t+1})|sq;r] = E[Q(X_{i,t+1})|sq] \geq 0,$$

obviously $E[X_{i,t+1}|sq] > 0$. Because this holds for any arbitrary status quo and because in general $E[X] = E[E[X|Y = y]]$, conditioning on every status quo shows that $E[X_{i,t+1}] > E[X_{i,t}]$. *Q.E.D.*

Theorem 10: *Suppose there are* N *departments, each with* n *agents. The Condorcet conditions hold. Judgment is crude and stationary; alternative generation is homogeneous and stationary.*

1. *If every dimension's new options are majority consistent, then* $\mathrm{E}[\mathrm{X}_{\mathrm{i,t}}]$ *is monotonically increasing in* n, *for all* i = 1, . . . , N *and all* t > 0.
2. *If every dimension's new options are majority inconsistent, then* $\mathrm{E}[\mathrm{X}_{\mathrm{i,t}}]$ *is monotonically decreasing in* n, *for all* i = 1, . . . , N *and all* t > 0.

Proof. Part (a) is proven; the proof of part (b), being symmetric, is omitted. Instrumental to the proof is the following lemma.

LEMMA 3. *Suppose there are* N *(odd) coins. Of these,* $\mathrm{k} \geq \frac{\mathrm{N}+1}{2}$ *are biased toward heads, and the remaining minority,* N − k, *are biased toward tails. The biases are equal: a coin of the "majority faction" will turn heads with probability* p ∈ (1/2, 1]; *one of the "minority faction" will turn tails with probability* p. *The coins are flipped independently. Then, for every fixed* N *and every fixed* $\mathrm{k} \geq \frac{\mathrm{N}+1}{2}$, *the probability that a majority of all the coins will turn heads is monotonically increasing in the bias,* p.

We can now finish the proof. Let $E[X_i]$ be based on n agents in each department and $E[X_i']$ on n' agents, where $n' > n$. Because judgment is crude and alternative generation is homogeneous, $E[X_{i,t}] = E[Q_0] + \Sigma_{s=1}^{t} \ \mu_{i,s}$ where $\mu = E[X_{i,s}|sq_{s-1} = (0, \ldots, 0)]$; similarly $E[X_{i,t}'] = E[Q_0] + \Sigma_{s=1}^{t} \mu_{i,s}'$. Hence it suffices to show that for an arbitrary s, $\mu_{i,s}' > \mu_{i,s}$.

As in the proof of theorem 9, let $q_{sq,r;t+1}$ denote the probability that given a fixed status quo in t, a new alternative is generated that is an improvement in r policy dimensions, $r = 0, \ldots, N$. Let $P_{sq,r;t}(n)$ denote the probability that a majority of departments vote to accept a new option (that is an improvement in r dimensions) over the status quo, given n decision makers in each department. Because the Condorcet conditions hold and judgment is crude, the probability that a majority of departments accept a new option depends only on the *number* of departments that would be better off with the new policy. The departments' identities are irrelevant. $P_{sq,r;t}(n')$ is defined similarly. Then

$$\mu_{t+1} = \sum_{r=0}^{\frac{N-1}{2}} q_{sq,r;t+1} \cdot E[Q(X_{i,t+1})|sq;R_{t+1} = r] \cdot P_{sq,r;t+1}(n)$$

$$+ \sum_{\frac{N+1}{2}}^{N} q_{sq,r;t+1} \cdot E[Q(X_{i,t+1})|sq;R_{t+1} = r] \cdot P_{sq,r;t+1}(n),$$

whereas

$$\mu_{t+1}' = \sum_{r=0}^{\frac{N-1}{2}} q_{sq,r;t+1} \cdot E[Q(X_{i,t+1})|sq;R_{t+1} = r] \cdot$$

$$P_{sq,r;t+1}(n') + \sum_{\frac{N+1}{2}}^{N} q_{sq,r;t+1} \cdot E[Q(X_{i,t+1})|sq;R_{t+1} = r] \cdot$$

$$P_{sq,r;t+1}(n').$$

By majority consistency, $E[Q(X_{i,t+1})|sq;R_{t+1} = r]$ is less than zero when $r < N/2$ and exceeds zero when $r > N/2$. Because lemma 3 establishes that $r < N/2 \Rightarrow P_{sq,r;t+1}(n) > P_{sq,r;t+1}(n')$ and $r > N/2 \Rightarrow P_{sq,r;t+1}(n) < P_{sq,r;t+1}(n')$, it follows that $\mu'_{t+1} > \mu_{t+1}$. *Q.E.D.*

Notes

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1. This was not the case in the application of incrementalism to budgetary research, where the controversy quickly became quantitative. However, with few exceptions (see Padgett 1980), the arguments underlying the quantitative research remained nondeductive.

2. Alternatively, one could assume that the agent finds it too complicated to think about this feasible set in any useful way.

3. Of course, the probability of *generating* a excellent new option may be smaller than that of generating a mediocre one, given a poor starting point. This is a separate matter, to be discussed shortly.

4. Kahneman, Knetch, and Thaler discovered that potential sellers (subjects who first received an endowment of coffee mugs) valued the mugs much higher than did potential buyers, who first received cash: the reservation price of the median seller was about double that of the median buyer (1990, 1332). Thus there were many unconsummated exchanges due to the "endowment effect"—the high valuation of the status quo endowment.

5. If Q_t is symmetrically distributed and the decision maker is just as competent as a fair coin (and no better), then $E[V_{t+1}|V_t = i] = i$: the process's expected value is exactly its current value. Such processes are known as *martingales.* March and Olsen (1984) suggested that policy processes are better understood as martingales than as independent trial processes. Their suggestion is based on the claim that policy-making is a path-dependent process (as are martingales), whereas independent trial processes are necessarily history-independent. Path dependence does not, however, imply the very special property of martingales, namely, that on average one stays where one is. Theorem 2 describes path-dependent processes that are generalizations of martingales.

6. I shall not explicitly model search behavior here but only investigate how different, exogenously fixed search strategies affect outcomes.

7. Theorem 3 may surprise incrementalists but may not surprise students of optimal search theory (see, e.g., Kohn and Shavell 1974). However, because the latter's work came after Lindblom's, it was unavailable to him. Moreover, in optimal search theory, decision makers are perfectly rational, so even if the work had been available to Lindblom, he might have thought it irrelevant. Indeed, to my knowledge, theorem 3 is the first result analyzing how increasing risk in the set of alternatives affects a boundedly rational agent and identifying the minimal level of rationality necessary for increasing risk to be beneficial. Because weak competence is a very low standard of rationality, it is interesting that this is all that is required.

8. For simplicity, it is assumed that the status quo is evaluated without error. Theorem 4 does not depend on this assumption, but the latter simplifies the proof.

9. More precisely, we assume that there are intervals $(-a_{1,t}, -a_{2,t})$ and $(b_{1,t}, b_{2,t})$ in the support of θ_t, where the probability density of θ_t is strictly positive. Of course, it is assumed that $-a_{1,t} < -a_{2,t} < 0 < b_{1,t} < b_{2,t}$.

10. Since the agent picks the new option if he or she evaluates it as being better than the status quo, $p_{i,i+k;t} = p(i < i + k + \theta_t) = p(0 < k + \theta_t) = p_{j,j+k;t}$.

11. Note that θ_t and θ'_t need not belong to the same class of random variables. For example, θ_t could be uniform, whereas θ'_t could be normal.

12. The Manhattan Project is an outstanding example of this point: the great advances in theoretical physics made this radical innovation possible.

13. Because theorem 5 describes the change in the relative benefits of radical search, weak competence is not required: $E[V'_t] - E[V_t]$ increases as (say) f rises even if $f < b$. Hence this result holds under more general conditions than does theorem 3, for in the latter, weak competence is absolutely necessary.

14. Note that evaluation of policies based on status quo search is flawless, since $d = 0$. Given this, the probability of correctly choosing a superior new alternative worth x over a status quo worth x_0 is just the probability that a normal random variable of mean x and variance σ^2 takes on a value in $(x_0, -\infty)$. A similar property holds for the probability of correctly choosing the status quo over an inferior new option. Note that because the normal is symmetrically distributed, the probability of choosing correctly always exceeds one-half. Thus in contrast to the more general error structure of theorem 4, which does not imply even weak competence, this specialized error structure implies strong competence, and the normal's symmetry implies insensitivity to the status quo as well.

15. In this regard Lindblom is part of an old intellectual tradition, going back to Condorcet, that has emphasized that more heads are better than one. For a valuable synthesis of the Condorcet tradition, see Grofman, Owen, and Feld 1983; for some recent extensions, see Ladha 1992 and Miller 1986. The more general idea of building reliable systems from unreliable parts was pioneered by von Neumann and is now a major field in engineering, much more prominent there than Condorcet models are in political science. Engineering reliability theory was introduced into political science by Landau (1969); see also Bendor 1985 and Heimann 1993.

16. Given the assumptions of equal competence and independent judgments, one can easily prove that under majority rule, a group of weakly competent individuals is itself weakly competent. A similar mapping holds for strong competence. Further, crude, homogeneous, and heterogeneous individual judgments translate into crude, homogeneous, and heterogeneous collective judgments. Hence theorems 1–5 and propositions 1–2 remain valid when there are N equally competent and independent decision makers.

17. This comparison is legitimate because Figure 4 is based on propositions 1 and 2, and the assumptions of both of these propositions are satisfied by the conditions of theorem 8.

18. "Analysis and evaluation are 'disjointed' in the sense that various aspects of public policy and even various aspects of any one problem . . . are analyzed at various points, with no apparent co-ordination and without the articulation of parts that ideally characterizes subdivision of topic in synoptic problem solving" (Braybrooke and Lindblom 1963, 105–6).

19. All subsequent numerical examples will presume a normalized status quo of $(0, \ldots, 0)$.

20. Clearly the inequality cannot hold for $r = N$, since for this subset the new options beat the status quo in all dimensions. Hence it is assumed that the probability of the event $r = N$ is zero.

21. For the reason indicated in no. 20, it is assumed that the probability of the event $r = 0$ is zero.

22. Another class of problems involves more conflict. For

example, suppose the new, equally likely options are (1, −1, −1) and (−1, 1, 1). (Note that dimension 1 is majority-inconsistent.) If the decision makers are competent, over time policy domain 1 will get worse and the other two will improve. Here, greater sophistication cannot help: specialist 1 is locked in a zero-sum game with the other two, thus precluding logrolls.

23. For example, if the following new alternatives are equally likely—(1, −1, −1), (−1, 1, −1), (−1, −1, 1) and (1, 1, −1), (1, −1, 1), (−1, 1, 1)—then they are majority-consistent. But the dimensions are not independent: the marginal probability of improvement on each dimension is $\frac{1}{2}$, yet the chance that all dimensions improve simultaneously is zero instead of $\frac{1}{8}$. Indeed, the dimensions are (pairwise) negatively related, indicating conflict across the policy domains.

24. Further, independence would mean that the agents would not be in conflict: the chance of generating the best option on one dimension would not be hindered by what was happening on the others. Trade-off frontiers require interdependent dimensions. (For example, with three dimensions, independence implies that the distribution of new alternatives looks like a box in three-dimensional space. The upper northeast vertex represents an alternative that Pareto dominates all others; hence no trade-off frontier exists.)

25. Further work will explore unbalanced growth. Numerical examples indicate that under some plausible conditions the policy domains that do not grow will be hurt by the increased size of the others. As in introducing people specializing in previously neglected policy dimensions (e.g., the environmental effects of building dams [Taylor 1984]), this may be a price that leaders are willing to pay.

26. Note that as the organization increases size, the number of policy dimensions (hence the number of departments) remains fixed. Hence, under the standard Condorcet conditions, it is the departments that move toward infallibility as they grow larger.

27. Even asserting, as a theoretical premise, that more incremental search generates better average alternatives does *not* establish that incremental search is superior to nonincremental. The reason is that radical search's greater dispersion remains an advantage. Thus incrementalism would trade-off a better average for smaller dispersion; the net effect is uncertain.

28. This presumes that something like theorem 2 holds, so that the average status quo in the incremental process improved over time. Otherwise, the less often policy changes, the better.

29. To save on notation, it is assumed here that the other $N - r$ dimensions are strictly worse off with a given alternative. The proof easily extends to the case wherein some options make no difference to some dimensions.

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WAR AND THE SURVIVAL OF POLITICAL LEADERS: A COMPARATIVE STUDY OF REGIME TYPES AND POLITICAL ACCOUNTABILITY

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We seek to answer the question, What effect does international war participation have on the ability of political leaders to survive in office? We develop a model of political reliability and derive seven related hypotheses from it that anticipate variation in the time a national political leader will survive in office after the onset of a war. Drawing upon a broadly based data set on state involvement in international war between 1816 and 1975, our expectations are tested through censored Weibull regression. Four of the hypotheses are tested, and all are supported by the analysis. We find that those leaders who engage their nation in war subject themselves to a domestic political hazard that threatens the very essence of the office-holding homo politicus, *the retention of political power. The hazard is mitigated by longstanding experience for authoritarian elites, an effect that is muted for democratic leaders, while the hazard is militated by defeat and high costs from war for all types of leaders. Additionally, we find that authoritarian leaders are inclined to war longer after they come to power than democratic leaders. Further, democratic leaders select wars with a lower risk of defeat than do their authoritarian counterparts.*

On 6 April 1982, six days after the Argentine invasion of the Falkland Islands, the *New York Times* correspondent in Buenos Aires gave this evaluation of the position of Argentine president Leopoldo Galtieri: "Political leaders here . . . agree he has greatly enhanced his political power and stature" by invading the Falkland Islands. At the same time Galtieri's political fortunes were in ascent, British prime minister Margaret Thatcher was being attacked by the British press for what was perceived as tardy reaction to the situation. In those few days, her political fortunes fell almost as much as those of Galtieri had risen. However, less than four months after Galtieri's stature had been ascendent he was out of office, while slightly more than a year after successfully repelling the Argentine forces, Thatcher and her party were returned to parliamentary power by a large majority.[1]

Of course, the Falkland's War was not a major conflict on the scale of, say, World War II or the Crimean War, and its value as a case from which we may generalize about the effects of war is limited. Nonetheless, it does serve as a striking example of the relationship investigated here—the effects of war on the tenure of political leaders and on their regimes among nations involved in war.

We first discuss the relationship between war performance and the subsequent fate of national political leaders. We then offer a model and seven related hypotheses accounting for what happens to leaders because of their war policies and describe our data and research design before reporting the results of our tests of four of the hypotheses. Because of presently existing data limitations, tests of three of the hypotheses must be postponed.

The research presented here represents an extension of our previous work on the political consequences of war in which we examined the effects of a state's initial position in a war (i.e., initiator or target), its outcome, and the costs of the war on the probability of the nonconstitutional overthrow of the state's political regime (Bueno de Mesquita, Siverson, and Woller 1992). Although the research reported here shares a broad set of interests in linkage politics with the earlier work (Rosenau 1969), it differs significantly in that the model used here is both more rigorous and more extensively specified than that used in the previous work. Moreover, because our present focus is on the survival *time* of the individual political leaders who were responsible for government policy at the point the state entered the war, our empirical tests are both more sensitive than those in the previous paper and speak more clearly to neorealist explanations of international politics and war. As we shall show, our results obtain even when we control for the dependent variable in the previous study, nonconstitutional regime overthrow. Consequently, the results here capture the strong additional effects on leadership survival that follow from our model, above and beyond the effects shown in our earlier analysis.[2]

WAR PERFORMANCE AND THE FATE OF LEADERS

Norpoth has observed that "war and economics have few rivals when it comes to making or breaking governments" (1987, 949). Our attention is directed at the "war" part of this assertion. Although many probably agree with the idea, the evidentiary base on which this assertion rests is both fairly narrow in terms of the range of time periods and governmental types studied and also, in some respects, ambiguous.[3] Data for the United States and the United Kingdom indicate that international crises and war can have an effect on the public's evaluation of political leaders. In the context of the United States, various studies (but most notably Brody 1992; Brody and Page 1975; Kernell 1978; Mueller 1973) have attempted to connect variations in presidential popularity to foreign policy events and participation in international crises. Although Mueller and Kernell portray presidents as generally benefiting from the short-run "rally" effects of foreign policy events, Brody's analysis draws out a more complex process in which a president may or may not enjoy a gain in popularity, depending upon a variety of factors, the most notable being the articulation of criticism by opinion leaders from either the media or the political opposition. However, there is little direct evidence bearing on the effect of war itself, although it is obviously worth pointing out that neither Truman nor Johnson was willing to hazard a try at reelection while engaged in wars that had divided the American public.

More broadly, with respect to the United Kingdom, Norpoth, using time-series methods, examined the impact of economic performance and the course of the Falkland's War on citizen ratings of Thatcher and the Conservative party between June 1979 and July 1985. He concluded that the independent effect of the Falkland's victory was worth between five and six additional percentage points to the vote for the Conservatives in the 1983 general election victory.[4]

All of these results are intriguing, but their domain is limited to the United States and Great Britain in the second half of the twentieth century. Absent is any broadly based theory or research on the general question of the effects of war involvement and war outcome on the political fortunes of the leaders who were responsible for them, even when those leaders presided over nondemocratic governments. This lacuna represents a major gap in our understanding of political accountability and the implications of such accountability for the selection of foreign policies.

Are political leaders and their regimes at greater hazard if they involve their nation in a war than if they do not? Is their political fortune affected by the outcome of the war? Does the effect, if any, fall equally across different types of political systems? Is the anticipation of domestic political punishment for failed policies an important element in shaping how nations relate to each other or, as suggested by neorealists, are these domestic factors minor features in the arena of international politics?

We contend that there are strong reasons to believe that a close connection exists between war and the domestic fate of governments and that the consequences of that connection can be and are anticipated by political leaders. Defeat in war almost always alters the loser's freedom of action by some measure, reducing the nation's autonomy over its own foreign policy or depriving the vanquished state of sovereignty over some portion of its citizens, territory, or national product (Morrow 1987). Compared to the often ambiguous outcomes of international conflicts and crises (see, e.g., Kernell 1978; Mueller 1973; Brody 1992) or even economic policy, evidence of loss from a war is much clearer to populations. Moreover, in nations without functional electoral systems, such evidence is far clearer to members of the elite, who themselves may have both the opportunity and motive for replacing leaders.

How can we assess the effects of war involvement and outcome on political leaders? One straightforward factor that would seem to be intimately tied to the welfare of any national leader is whether, given war participation, that leader's tenure in office is shortened or lengthened as a consequence of the state's performance in the war. Continuation in office may reasonably be seen as a reward, while removal from office (as opposed to natural death) in one way or another may be seen as punishment. We propose that leaders care about maintaining themselves in power—that they seek to maximize their reselection and, through the opportunities offered by continuing in power, to promote their own policy objectives. To achieve their objectives they must anticipate the effects their policies will have on the politically relevant domestic audience (Fearon 1994). Consequently, we expect that they will ex ante try to avoid policies that they believe will ex post foreshorten their hold over the perquisites of political leadership.

THE PROBLEM OF POLITICAL SURVIVAL

We begin with several assumptions. First, all politics is competitive. The issues over which—like the rules in which—the competition takes place differ across political units, and both the issues and the rules are subject to change. This portrait of politics is, of course, not remarkable.

Second, we assume that political leaders are intent on maintaining themselves in power and use the available tools of power and rules to accomplish this end. In like manner, we assume that all political leaders have opponents, most of whom are members of the leader's own political system, with their own ambitions for office. At the same time, leaders will often pursue policies that place them in opposition to those outside their own political system. Broadly speaking (and leaving natural causes aside), leaders then are subject to removal by their internal and external opposition or, quite possibly, some combination of the two. To be sure, we cannot dismiss instances in which a leader is removed by his or her "friends," who fear the costs to themselves of the leader remaining in power, but in this case, the friends have become opponents. Finally, given the opportunity, each opponent will be willing to pay a certain price to remove a leader.

Leaders, of course, recognize the existence of opposition and the designs of others on the office they hold. They consequently select policies to minimize the opportunities available to those seeking to remove them from power.

The ambition to remain in power, then, encourages political leaders to behave more responsibly than if they viewed the holding of office as a burden rather than as a prize (Bueno de Mesquita and Organski n.d.; Fearon 1994; Morgan and Bickers 1992). Enhancing the welfare of relevant constituents (to the extent that it is successful) removes from the opposition the most salient issues that can be used against a leader.

Several studies have tried to express a generic theory of the domestic politics relevant to foreign policy decision making (G. Allison 1971; Bueno de Mesquita, Newman, and Rabushka 1985; Bueno de Mesquita and Stokman 1994; Putnam 1988; Richards et al. 1993; Tsebelis 1990). We share with several of these approaches an interest in building on Black's (1958) median voter theorem and incorporating the notion that leaders want to be reselected. We also share the notion that voting is just a special case of the articulation of power or political influence and control, so that Black's theorem, suitably adapted, is relevant to policy formation in authoritarian as well as democratic regimes (Bueno de Mesquita, Newman, and Rabushka 1985; Bueno de Mesquita and Stokman 1994). It is just that in authoritarian regimes the median voter or pivotal power is drawn from a much smaller set of constituents than is true in democracies. In authoritarian regimes, then, as in democratic governments, the clique of leaders who can count on support from a majority of the relevant resources—whether they be guns, dollars, or votes—can expect to win office and retain it. With this in mind, we suggest the following model of governmental accountability for decisions about war.

Suppose that each nation consists of a set of stakeholders interested in influencing foreign policy decisions. In a democracy, this set may include everyone or nearly everyone in the society. In more authoritarian regimes, the set probably includes a more limited array of organized or unorganized interests. The military, politically active religious groups, business interests, government bureaucrats, and the population at large are a small sampling of such possible stakeholders. Each of these various groups engages in strategic maneuvers to promote their particular foreign policy agenda at the expense of alternative approaches to international politics.

At the end of the process of bargaining and possible logrolling, competing and allied internal interests come to a decision. We assume that the decision is equivalent to the policy stance of the median "voter," in other words, the policy preference of the median powerholder at the end of the bargaining process. Of course, since voting per se often does not take place or is not meaningful (especially in authoritarian societies), this median position is that policy supported by the individual or group that can count on the ability to mobilize more than half of the sum of all stakeholders' utilized power on behalf of its agenda against any possible challenge. The median stakeholder is the pivot around whom a winning coalition forms.

For the purposes of the present analysis, we do not elaborate a complete theory of domestic political decision making and its relationship to foreign policy. Instead, we describe a simplified model of implications that follow from such theories of domestic interest group competition. By keeping the model simple we naturally raise the prospects of having fairly robust results, and focus on broad generalizations as opposed to detailed nuances of the role of foreign policy and the retention of the leader in power. Additionally, because our model is rather general it cannot rule out some alternative explanations of the phenomena we discuss. That said, it is important to consider these caveats in the context of a model that is an integrated whole and generates a number of significant, testable hypotheses.

In describing these implications we begin by assuming that preferences across policy issues are single-peaked. This means that for each stakeholder the utility for any given resolution of an issue declines monotonically with the Euclidean distance from that decision maker's most preferred choice (i.e., the stakeholder's ideal point). We further assume that all utility functions are quadratic, reflecting the notion of declining marginal utility. This latter assumption is for ease of computation and does not materially affect our results.

Although many problems in international affairs and foreign policy are quite complex, involving the possibility of trade-offs and linkages across policy issues, we assume that this is not true of the most fundamental questions. For problems involving the risk of war, we assume that issues collapse to a single policy dimension having to do with the overall contribution of the putative policy to the welfare of the leadership's backers and opponents. This is broadly consistent with the realist notion that treats the state as a unitary actor.

Our view, however, differs from the realist approach in that the selection of policy options and the accompanying demands and actions taken in the international arena are not dictated by external, structural considerations. Rather, the choice of goals and actions is given shape by the domestic agenda of the leadership, as well as by the feasibility constraints of the external environment. Prudent leaders make choices that they think will help them retain power: they choose in such a way that they do not precipitate an internal overthrow of their authority. Consequently, their foreign policy goals may be seen as endogenous to their domestic political concerns rather than just to the international system's structure.

Because we have assumed single-peaked preferences and unidimensional issues on questions related to the threats of warfare, and because we propose that the coalition controlling a majority of political influence within a nation is expected to get its way, Black's (1958) median voter theorem can be applied. This means that the policy objective of the interested party located at the median of the distribution of power on the policy in question is the objective expected to prevail internally. The median power occupies the position that can, in head-to-head competition with any other proposed policy, muster a majority coalition. As such, it is the policy stance that maximizes internal political security, the position least susceptible to internal defeat.

With the median voter theorem in mind, we assume that on questions that involve the risk of war all nations can be summarized by examining the characteristics of three critical stakeholders:

1. the stakeholder or interested party, denoted as V, who controls the median power position;
2. the incumbent leader, called I; and
3. a challenger, called C, who wishes to gain control over the government's foreign policy.

So, with the state denoted as S we can say $\{V, I, C\} \in S$. V, of course, is itself an element in the preference distribution of all the stakeholders or interested parties in S. We assume further that policy objectives over which war is waged fall along a single policy continuum, denoted as $\mathbf{R}$, with $\{X_C^*, X_C, X_V^*, X_I, X_I^*\} \in \mathbf{R}$. (Terms with a superscript * are ideal points and belong to the actor named by the subscript. Terms without superscripts are the publicly taken policy positions of the subscripted actor.) The term X_C^* represents the ideal point, or most preferred foreign policy, of the challenger for power, and X_C expresses the actual policy position openly supported by the challenger in its attempt to woo the median stakeholder V away from supporting I. The other terms have analogous interpretations.

Incumbents can have an advantage over challengers in our model because they can earn political credit for their past performance or demonstration of reliability if they pursue foreign policies that satisfy V. This means that V can gain utility from the past performance of the incumbent, which is broadly consistent with the idea of retrospective voting (Fiorina 1981). Incumbents with bad records are more likely to be turned out; incumbents with good records from V's perspective have an edge in the ongoing campaign to remain in power. But incumbents also have a disadvantage because V bears the costs associated with the foreign policies pursued by the incumbent and V is not reluctant to pass judgment on I in response to these costs. I can accumulate negative credits as well as positive ones. Costs occur as a result of actions in wartime whether the war ultimately proves to be successful or not, as well as potentially arising as a consequence of policies by I that alienate V.

Let R denote the accumulated costs or benefits associated with the past performance of political leaders. We assume that the more constituents who have to be satisfied by a political leader, the smaller R is—and indeed that as the number of constituents rises so does the likelihood that $R < 0$. This is consistent with the notion of the coalition of minorities effect identified by Mueller (1973). The longer a leader has been in power, the greater the opportunity the leader has to alienate part of his or her coalition of supporters, gradually eroding the chances of holding onto support from the median voter (Powell and Whitten 1993; Rose and Mackie 1983). In authoritarian regimes, where fewer constituencies have to be satisfied, leaders are better able to fulfill the wants of their crucial backers. Consequently, $R > 0$ is probably true for authoritarian leaders, while $R < 0$ is more likely to be true for leaders in democratic states. In any event, whether positive or negative, it is likely that R for democratic leaders is smaller than R for authoritarian leaders. R_t is the benefits or costs from the leader's performance on the job at a specific time in the past (denoted by the index t), with $t = 0$ being the present.

If a current wartime policy is implemented and succeeds (i.e., the nation in question wins the war), we assume that the reliability benefits R are increased by $R_0 > 0$ but that if the policy fails, then $R_0 \leq 0$. R_0 is, then, one of the critical elements at stake for an incumbent engaged in a war. R_t decays over time so that recent demonstrations of reliability are more valuable to V (and therefore to I) than are demonstrations in the more remote past. Similarly, recent policy failures are more costly than old ones that have been survived. We denote this decay effect by discounting earlier demonstrations of competence or incompetence by d_t, with $0 < d < 1$, so that

$$R = R_0 + \sum_{t=n}^{1} d^t R_t.$$

The benefits of competence ($R > 0$) or the costs of incompetence ($R < 0$) are realized by I only so long as he or she remains in power. Consequently, when an incumbent is replaced, R returns to zero for the former incumbent. Since the challenger has not yet had an opportunity to demonstrate competence or incompetence, V expects $R = 0$ when the challenger first comes to power.

Incumbents, of course, serve only for a finite (though usually indeterminate) time. We denote this by specifying that R accumulates over the interval from the time when the leader first comes to power, $t = n$, to the present moment, $t = 0$. The leader's tenure in office, then, at the time a war starts, is the interval from $t = n$ to $t = 0$ (i.e., the moment the war starts). Assuming that R_t is a constant that decays in value at the rate d^t, then R is a logarithmic function of tenure in office that reflects the marginally declining impact of past successes or past failures on the current evaluation of I's job performance.

In addition to the costs or benefits associated with the leader's overall performance, we assume that there are direct transaction costs associated specifically with waging war. Let L denote the transaction costs or losses borne by the society (i.e., summarized by V) as a result of the implementation of wartime actions by I. These costs represent a burden of war that leaders must overcome if they are to be kept in office.

The fundamental dynamic in our conceptualization

TABLE 1

Utilities for the Incumbent (I), Challenger (C) and Median Voter (V) under Alternative War and Outcome Scenarios

	UTILITIES		
OUTCOME SCENARIOS	INCUMBENT	CHALLENGER	MEDIAN "VOTER"
Wins & retained	$-(X_I-X_I^*)^2 + R$	$-(X_C^*-X_I)^2$	$-(X_V^*-X_I)^2 + R - L$
Wins & replaced	$-(X_C-X_I^*)^2$	$-(X_C^*-X_C)^2$	$-(X_V^*-X_C)^2$
Lost & retained	$-(X_I-X_I^*)^2$	$-(X_C^*-X_I)^2$	$-(X_V^*-X_I)^2 + \Sigma_{t=n}^{1} d^t R_t - L$
Lost & replaced	$-(X_C-X_I^*)^2$	$-(X_C^*-X_C)^2$	$-(X_V^*-X_C)^2$

of domestic politics revolves around the expectation that incumbents wish to retain power and challengers wish to replace incumbents. This means that actor *I* wishes to remain more appealing to *V* than is *C*. However, *I* and *C* not only want power, they also have policy objectives of their own. That is why we have defined their ideal points as well as their public stance on the policy questions of the day. Thus our candidates for leadership may be pulled in two directions: to do what *V* wants and to do what they themselves want on foreign policy questions. They are not merely motivated by a desire for power and may be quite principled in terms of their policy interests. But when torn between personal preferences and constituent expectations, the successful political leader is likely to be someone who recognizes that politics is the art of the possible.

The political costs and benefits of alternative choices that are reflected by these assumptions are summarized as a set of utility values. Table 1 displays the utilities for *I*, *C*, and *V* under the four scenarios of interest to us:

1. *I* is expected to win the war it wages and *I* is retained in power;
2. *I* is expected to win but is removed from power anyway, being replaced by *C*;
3. *I* is expected to lose the war and is retained nevertheless; and
4. *I* is expected to lose and is replaced.

The incumbent, *I*, can be sure of retaining power only so long as *V* believes it is better off with *I* than with *C*. *I* remains the incumbent if *V*'s utility for retaining *I* is greater than *V*'s utility for replacing *I* with *C*. With *P* defined as *V*'s subjective probability estimate that *I* will win the war it is involved in, *V* will retain *I* in power if

$$P[-(X_V^* - X_I)^2 + R - L] + (1 - P)[-(X_V^* - X_I)^2 + \sum_{t=n}^{1} d^t R_t - L] > -(X_V^* - X_C)^2 \quad (1)$$

Several inferences can be drawn from expression 1. Solving for PR_0, which reflects the expected political stakes for both *I* and *V* from the war, we see that retention in office requires that

$$PR_0 > (X_I + X_C - 2X_V^*)(X_I - X_C) - \sum_{t=n}^{1} d^t R_t + L \quad (2)$$

The incumbent has control over several factors in expression 2. These include selecting events for which the probability of success is believed to be high and picking policies that are not so objectionable to *V* that the policies become an encumbrance to *I*'s retention of power. *I* naturally tries to pick X_I to ensure that the inequality in expression 2 is satisfied, while *C*, of course, picks its policy position to try and thwart *I*. Yet *C* and *I* are also constrained in selecting a policy because neither *I* nor *C* will wander so far from their respective ideal points that gaining or holding power is a pyrrhic victory.

It is evident from expression 2 that *C* has little incentive to locate itself at the same policy position as *I*. Being Tweedledum to *I*'s Tweedledee (Downs 1957) simply means that *I* will be retained if

$$PR_0 > L - \sum_{t=n}^{1} d^t R_t \quad (3)$$

It is evident from expression 3 that if $R > 0$, then, barring costs expected to be large enough to offset all of *I*'s reliability credits, *C* has no chance of removing *I* no matter how poor *I*'s chances of bringing the country to a victorious outcome in the war. Even if $P = 0$ (i.e., defeat is expected to be a sure thing), the expected costs must outweigh the credit for past performance accumulated by *V* in order for *C* to be chosen over *I*. Of course, if R_t is negative, *C* has an easier time removing *I*. Even in that case, however, rather than be Tweedledum, *C*'s best hope of gaining power is to support a position sufficiently close to X_V^*. Even in the worst case for *I*, when *C* adopts X_V^*, *I* can retain power provided that

$$PR_0 + \sum_{t=n}^{1} d^t R_t - L > (X_I - X_V^*)^2 \quad (4)$$

Clearly we see in expression 4 that *I* is constrained to stay relatively close to *V*'s ideal point if *C* adopts that position. *I* can drift away only to the extent that its past reliability and the expected reliability gains from the present war are large enough to offset its policy difference *and* the expected transaction costs from the war. If its past performance has accumulated costs rather than benefits, then, of course, *I* will have a more difficult time holding on to power, having to rely exclusively on the benefits derived from the current war.

Is it possible for *I* to prefer that *C* gain power rather than choose a policy stance X_I that is more distasteful to *I* than losing power to *C*? In order for *I* to prefer a government led by C over a government led by I, it must be true that

$$-(X_I - X_I^*)^2 + R < -(X_C - X_I^*)^2 \quad (5)$$

if I expects to win the war, or

$$-(X_I - X_I^*)^2 < -(X_C - X_I^*)^2 \quad (6)$$

if I expects to lose the war.

Expressions 5 and 6 suggest some consequential differences between democracies and authoritarian states, given our assumption that R for democracies is smaller than R for authoritarian states (including the prospect that $R < 0$ is more likely for democratic leaders than authoritarian leaders). Expression 5 implies that democratic leaders are more likely to leave office voluntarily than are authoritarian rulers. If $R > 0$, then it should be obvious that I would never pick X_I such that X_C is preferred by I to its own position. Consequently, I cannot prefer a government led by C to a government led by itself so long as $R > 0$ in our model, which presumably includes all authoritarian leaders and some democratic leaders. Authoritarian leaders can be expected to seek to hold office for life, never stepping aside on principled grounds. Some democratic leaders can be expected to behave quite differently, even choosing to lose office rather than pursue objectionable policies.

I can, of course, choose a different policy position than C, but the choice will be in favor of a policy closer to I's ideal point and never farther away. Then, the range of policy choices that I can make is constrained. C will do best, in terms of maximizing its chances of being selected to replace I, by picking V's ideal point as its own policy position (even though that is not C's ideal point), as we have already mentioned. I, then, can drift away from the median stakeholder's policy preference up to the limit of the value of the reliability benefits that I generates for V, less whatever costs are associated with I's war policy. If I's accumulated R values are negative, then I cannot drift away from V's ideal point, presuming that it is known to I. Avoiding war must thus be inherently better for I than waging war unless $R_0 > L$.[5]

It is evident, then, that X_I is endogenous, being chosen strategically (as is X_C) to facilitate I's retention of power and to maximize I's expected utility. X_C, naturally, is chosen by C to try to reverse the above inequalities in an attempt to induce V to prefer C to I. The threat of being replaced by C constrains I not to wander too far from X_V^*, while I's own policy concerns constrain the incumbent not to drift too far from its own ideal point. Leaders who want to retain power can rarely afford to hold an uncompromising commitment to the pursuit of the policies represented by their ideal point. Such "true believers" are unlikely to survive politically unless they happen to have the good fortune that their ideal point is the same as X_V^*.

In the scheme we have proposed, I can have a distinct advantage over C and can also suffer a distinct disadvantage from its actions. The advantage stems from its reputation for reliability if that is positive. In expression 2, the authoritarian incumbent's past record of performance decreases the size of the righthand side of the inequality, making it easier to stay in power even if the war is lost. The opposite is true for democratic leaders for whom $R < 0$. And the bigger the prospective stakes in the war (R_0) for I, the more likely it is that the incumbent will fight even with a small chance of success. These implications of our simple model give us the following initial hypotheses:

HYPOTHESIS 1. *The odds in favor of political survival increase as a function of the logarithm of the time that the leader has already been in office for authoritarian leaders, while the odds of survival increase less—or even decrease—for democratic leaders.*

HYPOTHESIS 2. *The greater the prospective benefits of the war* (PR_0)*, the more likely the incumbent will wage the war rather than resolve its differences through other means. Conversely, the smaller those prospective benefits, the less likely the retention threshold will be passed and, therefore, the less likely the incumbent will risk its position by fighting and the more likely the incumbent will be deposed if it does take the risk of fighting.*

Both of these hypotheses are testable. However, only the first one is central to the concerns addressed here. Consequently, we test hypothesis 1 and defer a test of hypothesis 2 to a future study focused on war behavior rather than leadership retention.

The incumbent must bear the burden for the failure of diplomacy and for the lost lives and property that are bound to result from war (L). This term, of course, makes it harder to keep power. The gains of reputation, if any, may be offset by the expected losses in the war. This suggests that the selection of wars to fight is itself endogenous. We have already seen that PR_0 influences the likelihood that a leader will be retained in office. The size of this term is within the control of political leaders to the extent that leaders can choose to resolve disputes short of war if the value of PR_0 is expected to be too small to lead to retention. Thus we have already seen one way in which war selection is endogenous to domestic political circumstances. Now we see that the endogeneity also extends to the impact that war costs are expected to have on domestic politics.

A leader can reduce the size of L by offering concessions to a foreign adversary in the hope of precluding a war so costly that it threatens to drive one from power. Likewise, one can eschew initiating a war expected to culminate in such high costs. Consequently, the wars we observe in nature are presumably a biased sample of the prospective wars that were considered and rejected. It follows then that the observed wars are those expected to have low enough costs that they would not jeopardize the leader's retention of power. This suggests two additional hypotheses:

HYPOTHESIS 3. *All else being equal, the greater the expected costs in war* (L)*, the more likely the incumbent will be replaced by the domestic political process.*

Hypothesis 4. *All else being equal, the greater the expected costs from a prospective war, the higher the probability that the leader will not engage in war but rather resolve international differences through other means, such as negotiations.*

Hypotheses 3 and 4, like 1 and 2, are testable. Hypothesis 3, like hypothesis 1, is focused on our central concern with the accountability of political leaders. Consequently, it will be tested here. Hypothesis 4, like hypothesis 2, is more oriented toward an investigation of dispute escalation than toward an evaluation of the survival of political leaders. We defer to a later study any tests of hypothesis 4.

The reliability variable in our model reveals several important features of incumbency. The longer a leader has been in power prior to the onset of a war, the greater the opportunity the leader has had to amass credit for reliability or to lose supporters as part of the coalition-of-minorities effect. The latter effect is more likely to arise the more dependent the leader is on multiple constituencies, while the former effect is more likely to be realized by authoritarian leaders who must satisfy more limited constituencies. Thus, all else being equal, the longer an authoritarian incumbent has been in power, the more likely it should be that the incumbent will be retained in office once a war begins, even if the war is lost. The beneficial effects of a long prewar incumbency should be significantly muted (and can even be reversed) in democracies relative to authoritarian leaders. This can be seen more clearly from expressions 7 and 8:

$$R - L > (X_I - X_C)(X_I + X_C - 2X_V) \qquad \textbf{(7)}$$

$$\sum_{t=n}^{1} d^t R_t - L > (X_I - X_C)(X_I + X_C - 2X_V) \qquad \textbf{(8)}$$

Expression 7 denotes the conditions under which *V* prefers to retain *I* if *I* wins the war while expression 8 denotes the conditions for retaining *I* when *I* loses the war. Of course the left side of expression 7 is strictly larger than the left side of expression 8, because $R_0 > 0$ in a victorious war, so that the incumbency advantage is, not surprisingly, greater if one is victorious. This suggests a fifth hypothesis:

Hypothesis 5. *Tenure in office has a greater beneficial impact on the political survival of incumbents expected to win their wars than on incumbents expected to lose.*

From expression 2, it is evident that the longer an authoritarian *I* (or a democratic *I* not suffering from the coalition of minorities effect) has been in power (and therefore the greater the accumulated reliability benefits) the smaller *P* can be and still satisfy the requirements for retention in office. In a comparative static sense this means that the longer the tenure of an authoritarian leader, the easier it is for that leader to believe that he or she can survive the political consequences of losing a war. Consequently, authoritarian "old-timers" in office can more readily afford to pursue foreign policies that represent a gamble, with a high risk of failure. Newcomers to power, conversely, cannot afford such boldness and are thus more likely to avoid high-risk gambles in foreign policy. All else being equal, then, long-surviving nondemocratic leaders should be more likely to wage losing wars (or wars in general) than incumbents who are newer to their positions. We state this as our sixth hypothesis:

Hypothesis 6. *The longer an authoritarian leader has been in power, the higher the probability that the leader will risk waging a war, including waging a war that ultimately is lost.*

Hypothesis 6 suggests that long-standing authoritarian leaders engage in riskier wars not because of any inherent flaw in their character but because of an inherent feature of the political conditions that keep them in power. Their country's political institutions facilitate their dangerous behavior. It is also evident that as the authoritarian incumbent's tenure in office grows longer, *I* can afford to drift away from policies preferred by *V* because of the cushion provided by its reputation for reliability among its limited constituency. Paradoxically, those who have been reliable to their key followers in the past can afford to be less reliable to them in the future. Recall that in our model *V* is prepared to retain *I* in power even if *C*'s policies are closer to those desired by *V* than are *I*'s, provided $R > 0$. *V* selects its leaders in terms of an evaluation of overall welfare not just on the basis of current policy stances. This provides *I* with the opportunity to shift its policies closer to its own ideal point and away from *V*'s preferences as *I*'s reputation for reliability grows with its tenure in office. This suggests our final hypothesis:

Hypothesis 7. *The longer an authoritarian leader has been in power, the more likely he or she is to pursue personal policy preferences rather than the policies of* V.

This final hypothesis, though interesting and a clear implication of our model, is, like hypotheses 2 and 4, reserved for a future study because it is not central to our concern with leadership survival.

Hypotheses 1, 3, 5, and 6 form the core of our present investigation. Each of these four hypotheses refers to a feature of leadership retention that links war behavior and regime type to domestic political considerations rather than to the high politics of a realist or structuralist view of international affairs. These hypotheses represent summary statements of more detailed implications of the basic model of war choices we have delineated. Some are intuitive, but some are surprising.

In particular, we believe it is surprising that longevity in office makes leaders, particularly authoritarian leaders, more prone to wage wars, especially wars they can expect to lose. We also think it is surprising that longevity facilitates political survival for authoritarian leaders more than for democratic elites, especially in light of the proposition that it also facilitates the waging of losing wars. But even the intuitively more apparent hypotheses are important to test. We should always bear in mind that intuition can be fickle or wrong. Simply because something *seems* to make sense does not mean that it reflects how the world actually works. Also, we should feel greater confidence in counter-intuitive propositions if they are part of a theoretical structure that yields many intuitively anticipated results. Finally, even when ideas seem intuitive, it is useful to pin them down within a logical structure so that we can see more clearly how they relate to other concepts and exactly how they relate to each other.

THE DATA

Hypotheses 1, 3, and 5 link the survival of political leaders after the onset of war to their prior tenure in office, expected costs, regime type, and the expected outcome respectively. To test these hypotheses, we require data that permit us to relate the length of time a policymaker is able to remain in power after war onset, the outcome of the war, the costs of the war, the prewar tenure of the leader, the openness of the political regime, and the expectations that those around the leader had with respect to that leader's continued ability to rule. Most of the data are fairly straightforward, and some of them are widely available.

The states participating in war between 1816 and 1980 are given in the well-known collection of the Correlates of War Project reported in Small and Singer's (1982) *Resort to Arms*. The data set not only reports on national involvement in all international wars between 1816 and 1980 with at least a thousand battle-related fatalities but also identifies the states that were the eventual winners and losers. From this list we exclude several groups of states. First, we exclude states that participated in wars beginning after 1975 because of uncertainty with respect to the casualty data (the need for which we shall explain). Second, because we are interested in the domestic political aspects of war involvement, we also exclude those cases in which the relevant political leader is deposed by the direct use of force by an external party.[6] For example, the cases of the Netherlands and Belgium in 1940 are excluded from the data, as is the case of Germany in 1945. However, the case of Premier Tojo, who led Japan into war in 1941, is included in the data set because he was driven from office well before the end of the war and the United States occupation. Finally, although we originally intended to include cases where the outcome was sufficiently unclear that it could be called a tie, all of these were associated with the Korean War. Rather than rest our analysis of this effect on only one war, we do not consider these cases. Our final data set consists of 191 cases of state war participation between 1823 and 1974.

Data measuring the duration in office of the political leaders who were the heads of the governments at the time the war began were derived from several sources. Our basic source of data was Spuler, Allen, and Saunders' (1977) *Leaders and Governments of the World*. These data were checked against the historical chronology given in Langer's (1972) *Encyclopedia of World History*, Bienen and van de Walle's (1991) *Of Time and Power*, and the *Cambridge Encyclopedia* (Crystal 1990, RR 42–67). Post-1965 data were also checked against *Facts on File*.

In selecting the relevant leader whose longevity in office is of interest, we identify the individual who was the *head of government* (as distinguished from the head of state, if relevant) at the time the war began. In the large majority of cases the head of government was the individual most responsible for formulating and implementing policy regarding war decisions. In democratic countries the identification was straightforward, with the prime minister, chancellor, or president (as appropriate) being the designated head of government. For nondemocratic governments more judgment was required. We tried to ascertain whether there existed a cabinet or council of ministers or a comparable entity serving under the head of state or whether there existed a legislative body concurrent with the head of state. In either case, we identified the leader of this cabinet or council of ministers or the leader of the legislative body as the relevant decision maker. If such a council, cabinet, or legislative body existed concurrent with a head of state, Spuler and his colleagues identified the relevant ministers and generally provided enough information to determine which individual was the chief minister or leader and thus, by assumption, was responsible for policy. Of course, in some instances there is nothing to substitute for historical knowledge, because the apparent constitutional form of the government had little to do with the actual exercise of political power. For example, we consider Stalin to have been the responsible political leader for the Soviet Union between 1928 and 1953, and Mao for China between 1949 and 1976, rather than anyone listed as being the leader of a council of ministers. Beyond this, in some instances the histories of the individual states were examined, and in a few cases these histories were particularly useful in determining who actually held political power.

From these data, the central items of information we ascertained were four: (1) the date the leader entered office; (2) the date the war began; (3) the date the leader left office; and (4) if the leader left before the end of the war, whether that exit was the result of death or a political removal.[7]

We are interested in ascertaining the effect of several variables on the survival of the political leader who takes a state into war. One of these is the outcome of the war. Here we focus our attention on wars in which there is a fairly clear winner and loser. We have taken the win/lose designations from the Correlates of War data set.

We are also concerned with the costs and benefits to a leader's political fortunes that result from longevity in office. We share with others the claim that democratic institutions impose political constraints (e.g., the coalition-of-minorities effect) on leaders to a greater degree than is true in authoritarian settings. Therefore, we assume that democratic leaders are constrained in their foreign policy choices by the acquisition either of reliability costs or smaller reliability benefits than is true for authoritarians over time, while authoritarian leaders are liberated in their actions by reliability credits that redound to them from the actions they take to satisfy their much more limited constituencies (Bueno de Mesquita and Lalman 1992; Maoz and Russett 1993; Morgan and Campbell 1991). Consequently, we are interested in the interactive effect of regime type with tenure in office as factors influencing political survival. The interaction of regime type and tenure is taken as our general indicator of *R*, the reliability cost or benefit in our model.

To calculate the impact of *R* from our model, we must specify whether each leader operated in a democratic or authoritarian setting. Gurr (1990) has undertaken an extensive survey of political systems in the nineteenth and twentieth centuries, reporting, among other things, a relatively rigorous measure of the extent to which various states were democratic. The scale runs from 0 (no democracy) to 10 (high democracy). All the states we cover are surveyed at the time of interest, so we have an estimate of the extent to which any state is democratic at, as nearly as possible, the time of the war onset. We measure the democraticness or authoritarianism of the institutions in each state by treating all cases that Gurr coded as 6 or above as democratic and those below 6 as authoritarian, coded as 1 and 0, respectively.[8] With this dummy variable, DEMO, in place, we create TENUREL * DEMO. TENUREL is the logarithm of a leader's total time in office prior to the war (plus 1), while TENUREL * DEMO is simply the product of DEMO and TENUREL.

In accord with our hypotheses, we anticipate that TENUREL * DEMO increases the hazard of being removed from office relative to that experienced by authoritarian leaders while TENUREL alone decreases the risk of removal. In other words, democratic leaders of states at war are expected to survive for a shorter time than their nondemocratic counterparts.

The transaction costs of war include losses in life and property and the attendant forgone opportunities that the destruction of lives and property entails. Although Organski and Kugler (1980) have been able to estimate some important dimensions of war cost for a few nations, we know of no data set that provides a usable measure of these costs for the number of nations with which we will deal. However, one reasonable alternative measure is available in the war lethality data contained in the Small and Singer data (1982, table 4.2). Small and Singer list for each nation's war participation the number of battle deaths per 10,000 population. This measure is particularly attractive because it is consistent across time and controls for population size. We expect this transaction cost measure to decrease the likelihood that a political leader will be retained in office.

At this point, it may be useful to lay out briefly the relationship between the hypotheses and the data. Hypothesis 1 indicates that the odds of political survival increase as a function of the logarithm of the time the leader has already been in office for authoritarian leaders (TENUREL) while the odds of survival increase less or even decrease for democratic leaders TENUREL * DEMO. The sources used to provide estimates of the postonset survival of leaders also provide the necessary data on the prewar tenure in office of each leader. Hypothesis 3 is testable against the reported battle deaths per 10,000 population, the form of which used here is the log because (1) the data are highly skewed and (2) increasing battle deaths probably have a decreasing marginal impact that would otherwise be exaggerated (Jackman 1993). In accordance with hypothesis 5, we expect that winning the war increases survival rates. The measure of war outcome is, of course, post hoc for leaders removed from office before the end of the war. It contains information that might not have been known to *V* at the time that the relevant constituents had to decide whether to retain or remove *I*. Here we treat the actual outcome as a post hoc indicator of probable expectations while the war was going on in those cases in which the leader was not retained to the end of the conflict.

Hypothesis 6 addresses expected changes in the conditions under which a leader would choose to wage a war. In particular, it indicates that the longer an authoritarian leader has been in power, the more likely the leader will choose to wage war, including high-risk wars that are lost. The likelihood of choosing high-risk wars (i.e., wars, on average, lost more often) is expected to be negatively associated with tenure in office for democratic leaders. To test this proposition, we examine the relationship between the logarithm of tenure in office (as suggested by the time discounting of past performance) and the outcome of the wars fought, taking into account whether the leader headed a democratic or authoritarian regime. If the hypothesis is correct, then the logarithm of tenure in office will be negatively associated with the likelihood of winning the war for authoritarian leaders and will be positive for democratic leaders. A second test examines the prewar tenure of authoritarian leaders whose nations engaged in war, comparing that tenure to the average total seniority of leaders in states that did not engage in warfare. If our hypothesis is correct, leaders of warring states should, on average, have already been in office before the war started for a longer time than is true for the total tenure in office of their counterparts in states that did not wage war. Authoritarian old-timers, recall, are hypothesized to pursue riskier foreign policies than their less senior counterparts.

To summarize, from hypotheses 1, 3, and 5 we have the following empirical expectations:

Leader's Post-War-Onset Political Survival

$$= a + b_1 \text{ TENUREL} - b_2 \text{ TENUREL} * \text{DEMO}$$

$$- b_3 \text{ (BATTLE DEATHS/10K)L} + b_4 \text{ WIN} + \epsilon;$$

and from hypothesis 6 we expect

$$\text{WIN} = c - b_5 \text{ TENURE}L + b_6 \text{ TENURE}L * \text{DEMO} + \epsilon;$$

and

$$\text{Average prewar tenure in warring states} > \text{average total tenure for authoritarian leaders of nonwarring states.}$$[9]

We add one additional test in which we control for nonconstitutional changes in the regime. In an earlier study (Bueno de Mesquita, Siverson, and Woller 1992) we reported a strong association between war performance and the survival of political regimes. Naturally, if a regime falls to domestic opposition, this may increase the likelihood that the individual key leader also falls from power. We are interested, therefore, in ascertaining the impact of our hypotheses on the survivability of leaders when we control for the effects of a nonconstitutional turnover in regime. The test adds the variable NONCON as follows:

Leader's Post-War-Onset Political Survival

$$= a + b_1 \text{ TENURE}L - b_2 \text{ TENURE} * \text{DEMO}$$
$$- b_3 \text{ (BATTLE DEATHS/10K)}L$$
$$+ b_4 \text{ WIN} - b_5 \text{ NONCON} + \epsilon.$$

EVENT HISTORY AND SURVIVAL ANALYSIS

Our approach to testing the specification of the model involves the application of survival analysis, often referred to as event history (by sociologists) or duration analysis (by economists). The dependent variable in the present case, the length of time a leader remains in power after the onset of the war, is exactly the kind of problem for which survival analysis was designed. The fundamental element of survival analysis is the estimation of the hazard rate, which may be thought of as the natural rate for the ending of some event or process. Here we are interested in the hazard rate faced by political leaders from the time their state enters into a war.

The hazard rate has two elements. The first is the underlying baseline rate of termination as if the event whose duration we are measuring is unaffected by anything. The second is the effect of the various covariates—specified as independent variables—that are seen as affecting the survival, in log-linear form, of the units of interest. In this case those units are the leaders.

There are two key advantages to event history methods over others. The first of these is that they allow us to include within the analysis cases that otherwise would be excluded or treated improperly. In the present instance, some of the leaders in our data set died in office through natural causes. The use of regression methods makes these cases problematic because their inclusion would inappropriately treat them as the political "deaths" that are of interest, while their exclusion removes from the estimate the information that they survived in office at least until their death. Event history analysis, however, allows us to include such information because these cases are treated as "censored"—that is, they are identified as lasting at least as long as the time until biological death. The contribution of such censored cases to the likelihood is then produced through the survivor function rather than the density function that is used on the noncensored cases.[10]

Second, it permits the hazard rate to change with the passage of time. The exact nature of this variation is, in fact, a critical element in distinguishing among survival models. While there are several such models, a graph of the hazard for our data shows it to be monotonically decreasing. Many survival models do not apply to a monotonically increasing or decreasing hazard, but the Weibull model accommodates such a pattern (P. Allison 1984). A plausible alternative to the Weibull is the exponential model in which the hazard is constant. A graphic method of distinguishing between the appropriateness of these models is to plot $\log(-\log(S(t))$ against $\log(t)$, where $S(t)$ is the survivor function defined by the Kaplan-Meier product-limit estimate and (t) is survival time (Kalbfleisch and Prentice 1980, 24). If the result is a straight line, the data may be judged to come from a Weibull distribution; but if the line has a slope of 1, the distribution is exponential. In the present case the scatter is on a straight line, but with a slope of less than 1, supporting the judgment of a Weibull with a decreasing hazard.[11]

DATA ANALYSIS

We turn now to an examination of the effects of the variables which compose our model.[12] The main question is, Does prior tenure in office, in combination with the authoritarianism or democraticness of the political system, the battle deaths per 10,000 population, and war outcome have the anticipated effect on the length of time that a political leader survives in office after the onset of the war? Table 2 reports the results of the maximum likelihood estimates based on censored Weibull regression for both the initial model and the one incorporating nonconstitutional overthrow of the regime. The coefficients are the estimated effect of the variable on the *hazard* rate of leaders; thus, negative values indicate a decreased hazard, or longer survival. The results reported in Table 2, column 1, reveal that all of the variables in the model have the predicted effect on political survival. Longer prewar tenure for authoritarian leaders and victory for all leaders extend time in office, while high overall battle deaths reduce subsequent time in office.[13] Our theory predicts that prewar tenure in office will be less advantageous to democratic leaders relative to their nondemocratic

TABLE 2

The Effect of War on Political Survival Time of Leaders: Censored Weibull Regression Test of Hypothesis 1, 3 and 5

INDEPENDENT VARIABLES	COEFFICIENT (1)	HAZARD RATE[a] (2)	COEFFICIENT (3)	HAZARD RATE[a] (4)
TENUREL	−.48** (.09)	.62	−.47** (.08)	.62
TENUREL * DEMOcracy	.33* (.16)	1.38	.36* (.16)	1.44
(BATTLE DEATHS/10K)L	.08* (.04)	1.08	.07* (.04)	1.07
WIN	−.28* (.16)	.75	−.26* (.15)	.77
NONCONstitutional overthrow	—	—	.51* (.19)	1.67
Constant	−.53** (.19)	—	−.62** (.20)	—
Sigma[b]	1.44 (.08)	—	1.43 (.08)	—
χ^2	34.2	—	39.8	—
Probability	<.01	—	<.01	—

Note: Entries in columns 1 and 3 are unstandardized regression coefficients with standard errors in parentheses. N = 191.
[a] On the hazard rate, see n. 15.
[b] On sigma, see n. 16.
* $p < .05$, one-tailed.
** $p < .01$, one-tailed.

counterparts. Since the coefficient for the effect of the length of prewar tenure for democratic leaders is estimated through the interaction TENUREL * DEMO, we obtain the estimate of the coefficient for just the democratic leaders by summing the coefficients for the interaction and the prewar tenure of all leaders (i.e., .33 − .48 = −.15). The coefficient of −.15 is greater than −.48, demonstrating that prewar tenure contributes less to the survival of the democratic leaders than of the authoritarian leaders, but is it, as we predict in the model, a significantly different effect? This can be shown by two F-tests. First, a test of the difference between this coefficient and the TENURE coefficient (−.48) yields an F of 13.17 ($p <$ .001). Second, we test the difference between −.15 and 0, and obtain an F of .76 ($p =$ .38). As our model predicts, the leaders of democratic states derive less advantage from prewar office holding than do the authoritarians; in fact, it is indistinguishable from no advantage whatever.[14]

The results are perhaps best understood as relative risks (or risk ratios), which are shown in Table 2, column 2.[15] In these expressions values above 1.00 (the baseline) indicate an increased risk that the leader would not survive in office, while hazards below 1.00 indicate that the survival rate has risen as the hazard has fallen. More precisely, the hazard's deviation from 1.00 is interpreted as the percentage increase or decrease in the likelihood of political survival resulting from the marginal impact of the independent variable, so that the relative effects of the variables can be discerned by the magnitudes of the hazards.[16]

Exponentiating the coefficient given for authoritarian leaders (−.48) produces a hazard of .62, which means that a one-unit increase in the length of their prewar tenure (an order of magnitude, since we are using the log of tenure) reduces the risk of postwar removal by 38%. In contrast, similar tenure for democratic leaders produces no significant benefit in survival (exp. [−.48 + .33] = .86, which, as we have seen, is statistically indistinguishable from zero). Thus regime type evidently makes an appreciable difference in the prospects of surviving a war politically, with democratic leaders placed at considerably higher risk than their authoritarian counterparts. Even victory does not enhance survivability as much as prewar tenure for authoritarians; nor does victory fully offset the increased hazard for democratic leaders. Victory reduces the overall risk of removal by 25%. Finally, all else being equal, it is easier for political leaders to survive low-cost wars than higher-cost ones. The risk of being turned out of office increases by 8% with each order-of-magnitude increase in the log of battle deaths per 10,000 population.[17]

Hypotheses 1, 3 and 5 are well supported by the evidence. What about hypothesis 6, which contains one of our more surprising expectations? Recall that this hypothesis indicates that authoritarian leaders who have been around a long time are better able to engage in risky foreign policies, even gambling on wars that have a relatively high probability of ending in defeat. Newer leaders and democratic leaders, by contrast, are not expected to take such large risks, and so pick and choose their fights more carefully, engaging in wars with a higher probability of leading to victory. The results of the logit analysis bear out the hypothesis. The actual result is

$$\text{WIN} = .49 - .35\ \text{TENUREL} + .77\ \text{TENUREL} * \text{DEMO},$$

with N = 191 and one-tailed probability = .002. The individual variables are also highly significant. The probability that the effect of TENUREL arose by chance is only .017. For TENUREL * DEMO, the probability that its effect is due to chance is only .007.

Hypothesis 6 implies a second, equally surprising result. Relatively short term authoritarian leaders have not had the opportunity to build up the reservoir of good will (*R*) among their few essential constituents that facilitates taking the risks of war. Democratic leaders are less likely than authoritarian leaders to have built up such a reservoir of good will after they have been in power for a long time. If democratic leaders are going to wage war, they are better off doing it early, before they have lost support as a result of the cumulative impact of the coalition of minorities effect. Consequently, on average we expect authoritarian leaders who engaged in war to have a longer prewar period in office than (1) the total tenure of all leaders who do not wage war and (2) democratic leaders who do wage war.

By moving slightly outside our data set, we can test these two expectations. Of our 191 cases, 106 are also to be found in the Bienen and van de Walle data set describing the political survival of 2,258 leaders around the world in the period since 1820. In this data set, the average total tenure of the nonwarring leaders is 3.32 years (N = 2,152), while the average total tenure of those leaders who ultimately engaged in war is 8.52 years (N = 106). The difference is highly significant, with t = 6.9. The average prewar tenure of all of the authoritarian leaders in our data set is 5.66 years, which is significantly longer than the total tenure of all the nonwarring leaders in the Bienen and van de Walle data set. The average prewar tenure of democratic leaders is only 2.57 years, which is significantly shorter than the prewar longevity or leadership experience of authoritarian leaders. The result is surprising, but consistent with our expectations. Long-serving authoritarian leaders are more likely to wage war than are relative newcomer democratic leaders. Democratic leaders are more likely to wage war early in their years in office, while their support is still high (Gaubatz 1991).

Hypothesis 6—like hypotheses 1, 3, and 5—seems to run directly counter to neorealist expectations and also to our general intuition. As such, it provides an additional basis from which to question the fundamental basis of neorealism and to suggest greater attention to the interplay between domestic politics and international affairs.

Before concluding, we examine the robustness of our results regarding hypotheses 1, 3, and 5 by controlling for the impact of nonconstitutional regime change produced by internal opposition. This test will help clarify the extent to which our model accounts for variations in leadership survival after controlling for regime change, a factor for which we have previously suggested an explanation (Bueno de Mesquita, Siverson, and Woller 1992). Table 2, column 3, contains the results of adding to our original model a dummy variable coded 1 for all the regimes that were overthrown by internal opposition either during the war or within three years of the war's end. As can be seen, even after controlling for nonconstitutional regime changes, the evidence in support of our hypotheses is quite robust. Nonconstitutional regime changes increase the risk of political removal by 67%, a very hefty effect. Still, the effects shown in the original model continue to obtain. The hazards, reported in column 4, show that authoritarian leaders continue to derive the same political benefits from their apparent ability to avoid problems such as the coalition of minorities. Similarly, winning and battle-related costs both continue to have significant effects of about the same magnitude as reported in the test that did not control for nonconstitutional regime change. In sum, our model's predicted effects are independent of our own earlier reported results for nonconstitutional regime change.

CONCLUSION

Our investigation has found that those leaders who engage their nation in war subject themselves to a domestic political hazard that threatens the very essence of the office-holding *homo politicus*—the retention of political power. The hazard is mitigated by longstanding experience for authoritarian elites, an effect that is muted for democratic leaders, while the hazard is militated by defeat and high costs from war for all types of leaders. Additionally, we find that authoritarian leaders are inclined to fight wars longer after they come to power than are democratic leaders. Further, democratic leaders select wars to participate in that have a lower risk of defeat than is true for their authoritarian counterparts. These results, which are implied directly by the specification of our model, obtain across a time span of over 150 years and encompass a broad spectrum of political systems and types of leadership removal. The evidence is consistent with the claim that decisions to go to war are endogenous to the domestic political setting of the leaders.

Such a result runs counter to expectations from neorealist theory. In that theory, war policies are endogenous to the international system and not to the domestic political situation. This is seen most clearly in Waltz's proposition that

> the elements of *Realpolitik*, exhaustively listed, are these: the ruler's, and later the state's, interest provides the

> spring of action; *the necessities of policy arise from the unregulated competition of states; calculation based on these necessities can discover the policies that will best serve a state's interests;* success is the ultimate test of policy, and success is defined as preserving and strengthening the state. (1979, 117, emphasis added)

We agree that policymakers care about the security of their state (though perhaps not necessarily as their paramount concern), and it is almost impossible to believe that the problem of maintaining or enhancing security does not enter into the calculations they make with respect to the policies that should be pursued. How does one square those facts with our assertion that internal political considerations are fundamental to external policy selection? The answer to this question depends upon what one takes to be the central assumption of neorealist theory.

If one proceeds from the basic neorealist assumption that *states* maximize their power to maximize their security and does not go further, then the theory is almost certainly false. However, if one extends the theory (in a way not previously done) by (1) assuming that *policymakers* want to stay in power for the rents, as well as for the policy opportunities thus afforded (Lake 1992) and (2) observing that declining security (as indicated here by war loss and costs) shortens time in power, then the linkage between internal politics and external policies is established. Thus the leader—whether president, prime minister, or president-for-life—who adopts policies that reduce the security of the state does so at the risk of affording his or her political opponents the opportunity of weakening the leader's grasp on power. Put differently, a leader's search for the security of the state intertwines with the search for policies that will maintain the leader in power against domestic opposition. The desire to remain in power thus provides the linchpin between the threats and uncertainties of the international system and the inevitable imperatives of fending off the domestic opposition.

Writing almost 25 years ago, James Rosenau (1969) lamented the fact that students of international relations did not have a well-developed framework—much less a well-developed theory—for linking political processes internal to the state with those that were external. In particular, he called attention to the absence of any theory that could account for the effect of foreign policy events on the tenures of political leaders:

> Consider the processes whereby the top political leadership of a society acquires and maintains its position of authority. To what extent are these processes dependent on events that unfold abroad? Under what conditions will the stability of cabinets and the tenure of presidents be reduced or otherwise affected by trends in the external environment? Are certain leadership structures more vulnerable to developments in the international system than others? Political theory presently offers no guidance as to how questions such as these might be researched and answered. (p. 5)

More recently, Putnam (1988) called attention to the linkages between international and domestic politics. Putnam's conceptualization of the logic of two-level games is certainly an advance over much of the past work on linkage politics, but although his concluding sentence is an admonition for empirical research, he fails to specify a model. The present research both specifies a model and offers data that are highly consistent with that model. With this knowledge in hand, we can no longer afford to treat domestic politics as ending at the water's edge, as neorealism is inclined to do. Foreign policy, instead, is better seen as intimately connected to the desire of leaders to maintain themselves in power.

Notes

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1. We are indebted to Gary Woller for calling this particular example to our attention.

2. There is a different way of putting this: all the political leaders used in this study left office, but only 36 of the regimes were overthrown by nonconstitutional means, and in 20 of these the responsible leader at the time of the entry into war had been removed from office *before* the overthrow of the regime.

3. The literature on the effects of economic performance on regimes and political leaders is quite large. Good summaries of the research are to be found in Lewis-Beck and Eulau 1985 and Norpoth, Lewis-Beck, and Lafay 1991. Although almost all of this work is within the domain of democratic political systems, research by Londregan and Poole (1990) demonstrates that military coups are more likely when economic performance has been poor.

4. However, Sanders, Ward, and Marsh (1991) argue that the Falkland's effect is exaggerated and that Thatcher's rise in the polls can be traced more clearly to economic policies and conditions.

5. It should be noted that we assume a game of complete and perfect information here. In later investigations we intend to examine the implications of uncertainty on the general effects suggested here.

6. To be sure, from a risk assessment point of view, policymakers cannot be indifferent to the possibility that a failed conflict policy may result in their removal by a foreign power. Indeed, it happens. In the present instance, however, we note that in the cases that would otherwise constitute our data base, removal by a foreign power took place only 19 times. However, our data probably understate the extent to which removal through this means occurs, because in many such cases the initiator is so "successful" that the casualties are not sufficiently numerous to qualify the event for inclusion in the war data set (e.g., the United States intervention in Grenada).

7. Not all deaths are neatly managed, because some are not natural. While Franklin Roosevelt died a nonpolitical death, Anwar Sadat did not. However, Sadat's assassins did not succeed in capturing power and replacing him with someone

who would bring Egypt's policies closer to their own. All political leaders are potentially subject to assassination, but the success of such attempts in the absence of a group able to seize power may be random. Consequently, in cases where assassins were not able to seize the state, we coded their departure as a "natural death." However, if a leader died as the direct result of a successful coup or revolution, the death was treated as a political removal.

8. One potential difficulty with this is that warfare sometimes changes governments. Few of such changes, however, are large enough to alter the state's score on the democracy index. For example, while the United Kingdom suspended elections during most of World War II, the democracy score remained unchanged at 10.

9. The two equations specified here are not intended to suggest a system of simultaneous equations but, rather, tests of hypotheses that follow directly from our model. Still, the dependent variable of one is an independent variable in the other so that it might be possible to conceptualize the argument as implying simultaneity. However, it should be noted that the factors hypothesized to explain the variable WIN are also independent variables in the first equation and so cannot be used as instruments for WIN. Having said that, we did test the argument as if there were a set of simultaneous equations. To do so, we calculated the predicted values of WIN from a logit analysis and substituted those predicted values into the first equation. Not surprisingly, the predicted values of WIN did not have a significant effect on the dependent variable, given that the predicted values were necessarily collinear with the effects of the remaining independent variables in the first equation. This had to be so because the remaining variables were exactly the same as the ones used to generate predicted values of WIN. As our results show, however, WIN itself is significantly related to the survival of political leaders even when the other independent variables are taken into account. This is the expectation derived from our model and suggests that additional factors explain war outcomes beyond those hypothesized here.

10. For a general introduction to survival methods, see P. Allison 1984. Applications in political analysis are growing. Some noteworthy examples of its use are to be found in the various papers of Warwick (1992a, 1992b, 1993). Also see King et al. 1990 and Hanneman and Steinback 1990.

11. Copies of the graph of the hazard and the plot of the integrated hazard against the log of survival time are available from Siverson.

12. The means, standard deviations, and ranges on the four main independent variables are

Variable	Mean	SD	Min.	Max.
TENUREL	1.34	.91	0[a]	3.45
(BATTLE DEATHS/10K)L	1.93	1.65	0[a]	6.32
DEMOcracy	.24	.43	0	1
WIN	.54	.49	0	1

[a] These values simply report numbers too small to register. For example, in the 1956 Suez War, the United Kingdom suffered 40 battle deaths, which, as a proportion of that state's population, is recorded in the data set at the value given above.

13. In keeping with the fact that our model leads to expectations about the direction of each relationship, one-tailed tests of significance are reported in the table.

14. The model we have tested does not include the main effect of democracy even though the interaction of democracy and tenure is present. We do this because we have no theoretical reason for including democracy. Nonetheless, we now report the same model including democracy as a main effect, and from the very small changes in the coefficients that attend this and the absence of a fit for democracy itself, we conclude that there is no empirical reason for including it either.

Variable	Coefficient (SE)
TENUREL	−.51 (.10)
TENUREL * DEMOcracy	.44 (.25)
(BATTLE DEATHS/10K)L	.08 (.04)
WIN	−.26 (.15)
DEMOcracy	−.17 (.31)
Constant	−.48 (.21)
Sigma	1.44 (.08)

15. Hazards are found by exponentiating the coefficients from the regression (P. Allison 1984, 28).

16. In Weibull regression, a shape parameter sigma describes whether the hazard is increasing or decreasing with time. When the hazard is decreasing, sigma has a value greater than 1.00. The value of sigma for our model is 1.44 (with a standard error of .08), so the hazard is decreasing, a result that is similar in character to that reported by Bienen and van de Walle (1991). In some statistics programs and in Greene 1993, the shape parameter is 1/sigma, in which case the effect of the shape parameter as increasing or decreasing the hazard relative to the baseline of 1.00 is the opposite of that given here.

17. Selecting the appropriate model for the overall hazard (in this case the Weibull) does not mean that other problems of misspecification are avoided. In ordinary least squares, diagnostics would be approached with the analysis of residuals. However, as Greene explains, "There is no direct counterpart to the set of regression residuals with which to assess the validity of the specification of the duration [i.e., survival] model" (1993, 722). Greene, nonetheless, does offer a test for specification, based on the use of "generalized residuals" (ϵ^2), to test the second moment restriction that $E(\epsilon^2) = 2$ (Greene 1993, 722–23; Lancaster and Chesher 1985b, 37). Since some of our observations are censored, the residuals are appropriately adjusted as:

$$\hat{e}(t) = \left[\begin{array}{l} \epsilon\ (t) \text{ if uncensored} \\ \epsilon\ (t) + 1 \text{ if censored.} \end{array}\right.$$

With the adjusted residuals, the second moment restriction is $s_e{}^2 = \Sigma\ (C_i/N)$, where $s_e{}^2$ is the sample variance of $\hat{e}(t)$ and $\Sigma\ (C_i/N)$ is the proportion of censored cases in the sample. The test statistic for the second moment restriction is implemented by running an ordinary least squares in which unity is regressed on $(\hat{e}_i - 1)^2 - C_i$, and all $\partial\mathcal{L}_i/\partial\theta_j$ where θ_j ($j = 1, \ldots, k$) represent parameters of the model. The test statistic is computed as N, the sample size, multiplied by the uncentered R^2 and under the null hypothesis has an asymptotic $\chi^2(1)$ distribution (Lancaster and Chesher 1985b). In the present instance, the value of the test statistic is .974, which is well below the 3.84 level necessary to reject the hypothesis at the 5% level. Additionally, we plotted the integrated hazard against the generalized residuals, the result of which was a 45-degree line characteristic of the Weibull (Lancaster and Chesher 1985a).

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REDISTRIBUTIVE POLITICS AND ECONOMIC EFFICIENCY

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The political process often compensates the losers from technical change or international competition in an economically inefficient way, namely by subsidizing or protecting declining industries instead of encouraging the movement of resources to other more productive uses. We find that a dynamic inconsistency in the game of redistributive politics contributes to this outcome. To achieve economically efficient outcomes, it is necessary that those making economically inefficient choices not be given offsetting transfers. But the political process distributes income on the basis of political characteristics, which are in general different from the economic characteristics that are rewarded by the market. We identify circumstances in which the inefficient choosers have desirable political characteristics and are therefore immune from threats of having to face the economic consequences of their choices.

Technical progress and international trade bring economic benefits to the nation as a whole, but both processes create some losers, namely those workers and owners of other factors whose incomes are tied to the industries or sectors that do not partake in the technical progress, or the ones adversely affected by international competition. The question then arises whether—and if so, how—the losers should be compensated out of the larger gains of the rest of the economy. Many would regard such compensation as ethically desirable; almost all would regard it as politically necessary or expedient.

This compensation often occurs in an economically inefficient manner. Subsidies or protection for declining industries have the effect of keeping labor and other resources in an activity where they are less productive than in alternative uses. The extra value forgone represents an economic cost to the nation as a whole. Such costs associated with inefficient policies to compensate losers are very large in practice. For example, Hufbauer and Elliott (1994) find that for the 21 industries in the United States that are most heavily protected, tariffs and quotas cost the consumers \$32.3 billion in the form of higher prices. Even after subtracting the producers' gains of \$15.8 billion and tariff revenues of \$5.8 billion, the net cost to the economy as a whole is \$10.7 billion.

Such economic inefficiency in the political process of compensation is our focus here. Our starting point is that the political process redistributes income on the basis of *political* characteristics. There are different views of what these are. Some analysts have emphasized membership in core ideological groups or parties, others have found centrality or swing voter status to be more important. In either case, the politically successful characteristics are not distributed equally across the population, and they generally do not coincide with the *economic* characteristics that are rewarded by the market. Politicians are unable to commit themselves to ignoring the political characteristics and making long-term promises to reward economically efficient choices. Therefore the political dynamics of redistribution interacts with the economic dynamics of occupational choice.

The economist's efficient solution to the problem of a declining industry would be to give lump sums to compensate the affected owners of factors specific to that activity (most notably the workers) and then leave all these factors to move to the best alternative use they can find. But in the political arena such policies depend on the mutual expectations of the political parties and the voters. The prospective losses typically extend over a longer period than one electoral cycle. The political parties will not give out capital lump sums up front to cover the whole period, because the recipients cannot credibly promise to go on voting for their benefactor parties in the whole sequence of elections. Therefore any such compensation must take the form of flows of transfers handed out in installments. Now the burden of credibility shifts to the political parties. What guarantees do the workers have that the promised future installments would continue? Future redistribution will reward those groups who possess suitable political characteristics at that time, not those who made economically desirable choices in the past. The workers will anticipate this when making their decisions of whether to relocate at some cost to themselves. Their knowledge that their receipts in the political process of redistribution are independent of their moving decision will dilute or even eliminate the economic incentive to move. The declining industry will be partially or even totally locked in.

We construct a simple, minimal model that highlights this issue and that produces a political equilibrium where the workers in a declining industry are totally locked in. We also point out more complex generalizations of the model where the lock-in is partial, but some economic inefficiency persists.

Similar phenomena can be found in many contexts of political economy, some of which we shall mention in our concluding remarks. But the theoretical development is better illustrated by focusing on just one example, and we have chosen the textile and apparel industry. Of course ours is an analytical model that makes a variety of simplifying assumptions to clarify the structure underlying redistributive politics. Any industry, textile and apparel included, has unique features that add interesting wrinkles to the basic story of redistribution. Therefore the textile example should be read as only an illustration of the workings of the political forces in our model. Fuller empirical testing of the model will require systematic data on the theoretically relevant parameters for a spectrum of industries and is beyond our scope here.

First, we shall give a very brief review of the literature on alternative explanations of economic inefficiency in redistributive politics. Then we shall set up our basic model, derive its results, and explain their intuition. We then introduce the textile and apparel example and conclude with some extensions or generalizations of the model and suggestions for further work.

RELATION TO THE LITERATURE

The most common explanation of the observation that declining industries are protected or supported in an inefficient manner goes at least as far back as Olson (1965), was labeled *client politics* by Wilson (1989), and runs as follows. The benefits of the protective policies go to a small proportion of the population with a large stake per capita. Such a group can more readily solve the free-rider problem involved in taking organized political action. The taxes that finance these benefits fall on the general population in very small per capita amounts. They either do not notice the costs, or do not find it worth their while to pay the costs of organizing in opposition to these policies. What is left unexplained in this story is the all-too-frequent use of economically inefficient means of channeling the political spoils. This argument has been criticized by others, including Arnold (1990), who pointed out that even if diffuse interests groups are ignorant or face the free-rider problem in pressing for their desired policies, enterprising politicians should solve both these problems for them—inform them and offer the beneficial policies and so gain their political support. This process may not work quickly or perfectly, but the criticism opens up the room for alternative explanations that are not based on imperfect information or free-riding.

Coate and Morris (1993) construct a model in which transfers to special interests take economically inefficient forms. They assume an asymmetry of information between the voters and the politicians. In fact, two kinds of asymmetries are needed—the voters must lack information about both the effects of policy and the predisposition of politicians. Moreover, the policy adopted in the equilibrium cannot be uniformly inefficient—there must be positive probability that it is, in fact, the better policy. Therefore their idea does not seem to explain well the situation of many declining industries, and our approach based on dynamic consistency has separate interest and explanatory power. Lohmann (1994) also considers a model of this type, in which some groups are given a priori advantages at overcoming the free-rider problem.

Our model is based on the idea that the political process cannot make long-run contingent commitments. A similar phenomenon in industrial economics is known as the *hold-up problem* (Williamson 1989). Consider two firms that enter into a joint venture, where one is specialized in providing "hardware" (e.g., in the form of a wiring network for cable television) and the other provides "software" in the form of cable programming. The hardware firm must move first, sinking some costs in the construction of a wiring network. In the absence of a complete and easily enforceable contract specifying how the profits are to be divided, the hardware firm will be vulnerable to demands from the software firm to renegotiate the division of profits after the network has been installed. At that point the hardware firm will have sunk its investment and be at the mercy of the software firm. The fear of such future hold-up will deter the hardware firm from investing in the first place, and a potentially profitable investment opportunity will fail to materialize. In this case, the problem is the credibility of the second firm's initial promise concerning the agreed profit split. In our case, the "hold-up" is caused by a more abstract political process rather than a concrete second party to a contract. Therefore we must construct and examine the equilibrium of this political process. But the effect is similar—an economically advantageous action is not taken.

Other analysts have considered the issue of time consistency in the context of policymaking. For example, Bulow and Rogoff (1989) consider the problem of renegotiating sovereign debt, noting that when a country promises to repay a debt, creditors will note that there is no third party available to enforce the contract. Staiger and Tabellini (1987) consider discretionary tariff policies in a similar way. Both these papers treat the government as a unitary actor with a time consistency problem. Fernandez and Rodrik (1991) argue that the losers from trade openings and technological progress are ex ante much more certain of their losses than the winners are of their gains. They show that this partial veil of ignorance may enable the losers from reform (e.g., trade liberalization) to assemble an electoral majority in opposition to reforms that ex post would have improved the lot of a majority of voters. Alesina and Tabellini (1990) note that with an intertemporal budget constraint, and some uncertainty about the party allegiance of future governments, the party in power may choose to "tie the hands" of its successors by incurring large debts, thereby raising the costs to future governments of spending on programs the present government views as undesirable. Among the four papers just discussed, only Alesina and Tabellini examine the consequences of competition between parties, and their result stems from policy-motivated parties trying to tie one another's hands, rather than competing in pursuit of votes.

THE MODEL

A minimal framework suitable for the development of the idea of dynamic inconsistency in redistributive politics is as follows. An industry is suffering from economic decline, and its workers are considering whether to move to another industry or location or both. Moving will bring them higher incomes, but to move they must incur various costs, such as time and money spent on retraining, job search and relocation, search for housing and medical arrangements at the new location, some social or psychological costs of separation from family and friends, and so on. In addition, both the stayers and the movers participate in the political process, with opportunities to get transfer payments or liabilities to pay taxes. We assume that the political process takes the form of a vote held *after* the decision to stay or move has been made. This captures our point that economic gains and losses extend over a period longer than one electoral cycle and that compensation policies cannot be credibly committed in advance.

The workers' occupation decision will look ahead to all these consequences: economic, psychological, and political. A rational decision maker will calculate the future consequences and work backward to the current decision. Accordingly, we begin with the final stage—the political process of income redistribution—and then go on to the occupation choice decision.

The argument can best be stated in a simple and focused way in a model.[1] The participants in the model are (1) two political parties, called Left and Right for sake of definiteness and labeled $k = L, R$ respectively, and (2) a continuum of voters, who split into a finite number n of interest groups labeled $i = 1, 2, \ldots n$. The total number of voters in group i is N_i, and the total voting population N is the sum of these N_i.

While the law is supposed to be no respecter of persons, lawmakers have considerable leeway in targeting benefits to groups. See, for example, the colorful list of tailor-fitted "transition rules" to the Tax Reform Act of 1986 detailed by Birnbaum and Murray (1988, 147, 241–43). So we allow the parties to condition their promises of transfers on group identity. The main distinguishing characteristic of these groups is their industry or occupation. Suppose that in the absence of a transfer policy, each member of group i would have income y_i.[2] When given a net transfer t_i (taxes count as negative transfers), the disposable income or consumption becomes $c_i = y_i + t_i$.

Parties' Strategies and Voters' Preferences

Each party proposes a redistributive policy. Party L's policy is a list (vector) of transfers $T_L = (t_{iL})$ to the different groups. These imply the consumption amounts

$$c_{iL} = y_i + t_{iL},\ c_{iR} = y_i + t_{iR}. \qquad (1)$$

The policies are required to satisfy the budget constraint[3]

$$\sum_{i=1}^{n} N_i t_{iL} = 0,\ \sum_{i=1}^{n} N_i t_{iR} = 0. \qquad (2)$$

Note that the policies take the form of lump sum transfers of money. Therefore they do not distort decisions to purchase commodities, as sales taxes would, or to supply the hours of work in any particular occupation, as would proportional taxes or subsidies on wage income. Because our focus is on the distortion in the occupation choice decision, it behooves us to show that policies can distort this process even when they impose no deadweight losses of any other kind. We shall discuss the effect of introducing such distortionary taxation later.

The policies constitute binding promises as far as the immediate future is concerned: a party that wins an election on the basis of a stated policy is required to carry out that policy in office. In a dynamic context, however, promises are not binding across election cycles; that is, a policy declared in course of the 1996 campaign is not binding when the 2000 campaign comes along, or thereafter.

Voters care about their disposable incomes and may have ideological or historical attachments to one or the other of the parties. For the general model, we need only specify the result of all these considerations, namely the proportion of people in each group who will vote for one of the parties (e.g., Left) in response to the disposable incomes they will get as a result of the two parties' declared policies. We write this as $P_i(c_{iL}, c_{iR})$. The label i on the function symbol indicates that different groups may respond differently to policies that promise the same disposable incomes, because their members have different characteristics and preferences, including any historical or ideological party attachments. Of course the proportion voting for Right is $1 - P_i(c_{iL}, c_{iR})$.

The proportion of any group that votes for party L should increase if the consumption that would accrue to members of that group as a result of party L's victory increases, and decrease if the consumption that would result from party R's victory increases. Therefore the function $P_i(c_{iL}, c_{iR})$ is increasing in its first argument and decreasing in the second argument. We denote its respective partial derivatives by the subscripts L and R, thus

$$P_{iL}(c_{iL}, c_{iR}) > 0,\ P_{iR}(c_{iL}, c_{iR}) < 0.$$

This general formulation closely follows Cox and McCubbins (1986) and Lindbeck and Weibull (1987). The only additional feature is that we have separated out for explicit attention the original incomes y_i from the other characteristics because they are crucial in the individuals' consideration of occupational choice to be modeled soon.

The "Swing Voter" Case. A special case of this general formulation gives some useful additional insight and intuition by permitting explicit solutions. It corresponds to the "swing voter" view of political characteristics mentioned earlier. This is the formal model of Lindbeck and Weibull (1987), as extended and developed by Dixit and Londregan (1995). In this view, the voting decisions of individuals are made by trading off the economic benefits promised by the parties against attachments to the noneconomic or ideological positions of the parties. Moreover, within each group there is a considerable heterogeneity of ideological attachments.

We model the voters as a continuum distributed along the real numbers: a voter located at X has an innate preference X for party R over party L. The voters also care for economic material benefits, measured by consumption c. We assume that the utility of a member of group i with consumption c is given by

$$U_i(c) = \kappa_i c^{1-\epsilon}/(1-\epsilon), \tag{3}$$

where the parameters κ_i and ϵ are both positive. We shall discuss their significance shortly. The marginal utility of an extra dollar of consumption for a group i member is $U_i'(c) = \kappa_i c^{-\epsilon}$. As c increases from 0 toward ∞, the marginal utility falls from ∞ toward 0. A 1% increase in c causes an ϵ% fall in the marginal utility.

As indicated in equation 2, voters' consumption depends on who prevails at the polls. Voters of type i weight the increased utility of consumption offered by party L against their innate preference for party R, voting for party L if the utility increase is sufficient: $U_i(c_{iL}) - U_i(c_{iR}) > X$. Define the critical value or "cutpoint" X_i by

$$X_i = U_i(c_{iL}) - U_i(c_{iR}). \tag{4}$$

Then all the members of group i with values of X less than X_i will vote for party L, and all the rest for party R. We denote by $\Phi_i(.)$ the cumulative frequency distribution of members over the range of X, so the proportion to the left of X_i is $\Phi_i(X_i)$. Thus we have an explicit functional form for the voting proportion function P_i that was introduced earlier, namely

$$P_i(c_{iL}, c_{iR}) = \Phi_i[U_i(c_{iL}) - U_i(c_{iR})]. \tag{5}$$

If party L offers a marginal extra dollar to each member of group i, differentiation of equation 4 shows that the cutpoint X_i will shift in its favor by $U_i'(c_{iL}) = \kappa_i(c_{iL})^{-\epsilon}$. Thus the voters' trade-offs between political ideology and economic benefit is captured in terms of two simple parameters. The ϵ parameter captures the degree of diminishing returns to private consumption. Values of ϵ near zero imply that even wealthy individuals remain very sensitive to offers of income redistribution. For large values of ϵ, the willingness to compromise one's party affinities for more private consumption benefits falls rapidly with increased income. Thus, with large ϵ, the parties can attract the votes of the poor relatively more easily by offering them small economic benefits. We assume that all groups have the same ϵ, because generality in this respect merely adds algebraic complexity without added insight. The parameter κ_i measures the relative importance of consumption as against the policy position, and here possible differences across groups do make a substantive difference to the analysis. A higher κ_i means that an extra dollar of consumption offered to group i shifts its cutpoint by a greater magnitude. In other words, group i is more responsive to promises of economic benefits. Thus we may think of κ_i as measuring how "apolitical" (or greedy) the voters of a group are.

The Voting Equilibrium

We assume that each party tries to maximize its vote total. This again follows the literature we have cited. An alternative line of the literature assumes that each party wishes to maximize its probability of winning. We believe that parties do care about the margin of victory or about keeping a larger minority presence if the victory goes to the other party. Moreover, the alternative formulation would yield qualitatively similar results at the greater algebraic cost of having to introduce uncertainty into the model.

Given the policies T_L and T_R and the resulting consumption amounts, the vote totals of the two parties are

$$V_L(T_L, T_R) = \sum_{i=1}^{n} N_i P_i(c_{iL}, c_{iR}),$$

$$V_R(T_L, T_R) = \sum_{i=1}^{n} N_i[1 - P_i(c_{iL}, c_{iR})]. \tag{6}$$

Each party chooses its strategy to maximize its vote, bearing in mind its own budget constraint and taking the other party's strategy as given. A pair of strategies (T_L, T_R) such that each party's strategy is the best response to the other's strategy is a Nash equilibrium of the electoral game. We now proceed to characterize it.[4]

Consider party L's choice of $T_L = (t_{iL})$ to maximize its vote, as given by equation 6, subject to its budget constraint given in equation 2. Define the Lagrangian expression

$$\mathcal{L} = \sum_{i=1}^{n} N_i P_i(c_{iL}, c_{iR}) - \lambda_L \sum_{i=1}^{n} N_i t_{iL},$$

where λ_L is the Lagrange multiplier.[5] Differentiating the Lagrangian with respect to each t_{iL}, we have the first-order conditions[6]

$$\frac{\partial \mathcal{L}}{\partial t_{iL}} = N_i[P_{iL}(c_{iL}, c_{iR}) - \lambda_L] = 0. \tag{7}$$

These conditions can be written more simply as

$$P_{iL}(c_{iL}, c_{iR}) = \lambda_L \quad \text{for all } i. \tag{8}$$

Similarly, writing $\mathfrak{R}$ for party R's Lagrangian and λ_R for its Lagrange multiplier, its first-order conditions are

$$\frac{\partial \mathfrak{R}}{\partial t_{iR}} = N_i[-P_{iR}(c_{iL}, c_{iR}) - \lambda_R] = 0 \tag{9}$$

or more simply,

$$-P_{iR}(c_{iL}, c_{iR}) = \lambda_R \text{ for all } i. \tag{10}$$

The Nash equilibrium is found by solving the first-order conditions (equations 8 and 10) together with the budget constraints (equation 2) and the consumption definitions (equation 1). We shall illustrate an explicit solution for the example with special functional forms, but for the general case we can characterize the essential features of the solution without finding its full details.

The important point is that the equations primarily determine the consumption quantities (disposable incomes) (c_{iL}, c_{iR}). To see this, write the budget constraints in using the consumption quantities. For $K = L, R$, we have

$$\sum_{i=1}^{n} N_i c_{iK} = \sum_{i=1}^{n} N_i(y_i + t_{iK}) = \sum_{i=1}^{n} N_i y_i \equiv Y, \tag{11}$$

where Y denotes the aggregate income of all the groups. Therefore the equation system defines the values of all the (c_{iL}, c_{iR}) given the aggregate national income Y. The effect is just as if each party proposes to collect together this whole income and then divide it up for consumption among the individuals in the different groups.

This has two consequences that will prove important in what follows. One is that the number of members in your group matters only through the budget constraint. Note how the N_i cancels out the first-order conditions in equations 7 and 9. In particular, if a group is artificially split into two, leaving unchanged their political attributes as encapsulated in the proportional voting functions $P_i(c_{iL}, c_{iR})$, then their disposable incomes will remain unchanged as well. (For a more detailed discussion of how sizes affect transfers, see Dixit and Londregan 1995).

The second important consequence is that an individual's earned income makes little difference to his or her own eventual consumption. This income is effectively thrown into a common pool and then shared out according to the rules of the political game, and the individual's own share from his or her earned income is almost zero. Almost none of any increase in his or her own personal earned income comes back to the individual; it is effectively taxed at an almost 100% rate. That is the crucial reason why, when next we consider the first stage of the game where people make their occupational choices, every individual's incentive to seek an economically superior alternative is diluted to the vanishing point.

The "Swing Voter" Example. We can obtain an explicit solution for the swing voter example. Using the forms (5) for the proportion of L-voters, and (3) for the utility function, the first-order condition (8) for party L becomes

$$\phi_i(X_i)\kappa_i(c_{iL})^{-\epsilon} = \lambda_L, \tag{12}$$

where $\phi_i(X_i) = \Phi_i'(X_i)$ is the density function corresponding to the cumulative distribution function $\Phi_i(X_i)$.

We obtain the equilibrium consumption amounts by solving for c_{iL} in the system of equations given by (12) and the budget constraint (11). This yields

$$c_i = \frac{\pi_i}{\sum_j N_j \pi_j} Y$$

where to simplify notation we have defined

$$\pi_i = [\kappa_i \phi_i(0)]^{1/\epsilon}.$$

The solution is as if the government collects all the output Y that is produced by the whole electorate and then divides the result, each person getting a share proportionate to his or her group's π_i. Therefore the π_i serve as measures of the groups' political power in achieving redistributive benefits. Most importantly, groups with high κ_i (greater willingness to trade off ideological preference in response to promises of economic benefits) and high $\phi_i(0)$ (greater density of members near the center of the ideological spectrum) have more power. (For a more detailed exposition of this special case of the "swing voter" model and its place in the literature, see Dixit and Londregan 1995.)

The model of Cox and McCubbins (1986) takes a different view, emphasizing "machine politics." For example, for many years, the urban ethnic politicians directed most of their patronage to their fellow ethnics. The argument is that each party can more efficiently supply transfers to its core supporters, because it can better identify their needs. (For a general approach that encompasses the "machine politics" and "swing voter" models and clarifies the reasons for the differences in their findings, see Dixit and Londregan 1995.) In our basic model, the difference is not important. What matters is that in each case redistributive success is primarily determined by *political* considerations, not *economic* ones.

The Location Decision

Suppose one particular group (e.g., number 1) is the declining industry. Those who stay in it will have a low per capita income y_s, and those who move will have a higher per capita income y_m. The move could be to one of the other existing industries (2, 3, etc.), or it could be to a totally new occupation. We label the subgroups of movers and stayers s and m, and write their numbers as N_s and N_m, with $N_s + N_m = N_1$, the original number of members of group 1. Of course the values of N_s and N_m must be determined as a part of the equilibrium.

If the individuals who moved could keep their gain $y_m - y_s$, and if their costs of moving were small enough, then they would move ($N_s = 0$), and this would be an economically efficient outcome. But we will find that the political process provides an alternative—and seemingly more attractive—form by which individuals can overcome their economic loss from staying in industry 1.

When the political game takes place at stage 2, the conditions that determine its equilibrium are as before (i.e., equations 2, 8, and 10) but now applying for $i = s, m, 2, 3, \ldots, n$, instead of $i = 1, 2, \ldots, n$. The crucial question is how the voting proportion functions for the new subgroups, $P_s(c_{sL}, c_{sR})$ and $P_m(c_{mL}, c_{mR})$ relate to the function $P_1(c_{1L}, c_{1R})$ for the original group 1. That depends on whether the movers are a representative sample from the original group 1 as regards their historical or ideological party affiliations or the like. A "neutral" assumption that provides a benchmark is that they are indeed such a random sample. We shall assume this neutrality for now and discuss it in the context of our textile example later.

Under this assumption, the functions $P_s(c_{iL}, c_{iR})$ and $P_m(c_{iL}, c_{iR})$ are the same as each other, inasmuch as each is the same as the original function $P_1(c_{iL}, c_{iR})$. Then the equilibrium values of (c_{sL}, c_{sR}) and (c_{mL}, c_{mR}) are also equal to each other. Note that their common value is not the same as what would prevail if the original group 1 stayed as an entity, because when some people move, the society's aggregate output changes, and this alters the equilibrium consumption of all groups including s and m. But the important thing is that movers and stayers get the same treatment: $c_{mL} = c_{sL}$ and $c_{mR} = c_{sR}$.

We see the result explicitly in the swing voter case. If one person switches from staying to moving, total output Y increases to $Y' = Y + (y_m - y_s)$. This person's consumption changes from c_s to c'_m, where

$$c_s = \frac{\pi_1}{\Sigma_j N_j \pi_j} Y \qquad c'_m = \frac{\pi_1}{\Sigma_j N_j \pi_j} Y'.$$

Notice that our neutral assumption that the mover and stayer subgroups have the same political power as that of the original group 1 implies that $\pi_m = \pi_s = \pi_1$. Therefore

$$c'_m - c_s = \frac{\pi_1}{\Sigma_j N_j \pi_j} (Y' - Y) = \frac{\pi_1}{\Sigma_j N_j \pi_j} (y_m - y_s).$$

We can now define an implicit tax rate τ on this person's additional income, namely, 1 *minus* the ratio of increased consumption to increased output, or

$$\tau = 1 - \frac{\pi_1}{\Sigma_j N_j \pi_j}.$$

The main thing to note is that this tax rate is very close to unity; almost all of the individual's contribution to aggregate income will be taxed away. For example, if all groups have roughly equal political powers π_i, then $\tau = 1 - 1/N$, where N is the total number of voters, typically in the millions. The case with the smallest implicit tax rate (the biggest effect on consumption) arises when only group 1 has any political power, so $\pi_i = 0$ for $i = 2, 3, \ldots, n$. Then $\tau = 1 - 1/N_1$, but N_1, the number of original workers in the declining industry, will still typically be in the thousands.[7]

Let the individual's moving costs be k, measured in utility units. If this individual could keep all the gain from moving, then he or she would move if $U(c_s + y_m - y_s) - U(c_s) > k$. Moreover, if this condition is met, then some of this individual's gain could be taken away and redistributed to others, leaving him or her and everyone else better off. In economists' jargon, the move will be a Pareto improvement.

When the political process cannot commit to this superior outcome and produces the equilibrium with its implicit tax τ, the individual will move only if

$$U(c_s + (1 - \tau)(y_m - y_s)) - U(c_s) > k. \qquad \textbf{(13)}$$

As τ is nearly unity, the utility gain from moving, shown on the left-hand side of the inequality, will be very small, so that even a small positive cost k will be enough to lock the worker in her current occupation. Notice that if the value of τ were lower, the lock-in would be partial, with some, but not all, of the worker's incentives to move remaining in the post-tax equilibrium.

The equilibrium of the dynamic game is now fully characterized. The economy is locked into a situation in which industry 1 is preserved intact and its members receive transfers that to some extent compensate them for their losses. The economy's aggregate income is lower than it could have been, but the move to a better outcome is stalled because the process cannot credibly either promise to members of group 1 any inducement to move to the superior occupations or (what amounts to the same thing) threaten those who fail to move that they will be left with their low incomes. The situation is as if almost 100% of the income increase earned by the movers is taxed away to be distributed to other groups according to their political characteristics.

It may seem obvious that a near-100% effective tax rate on movers severely distorts their occupational choices. But the argument is more subtle. Neither party *wants* to levy a 100% tax as such. But when the future election comes around, each has to compete with the other party and bid for political support. In the Nash equilibrium of this game, both parties end up offering transfer payments in a way that is tantamount to a 100% tax. Observers of the game and participants alike may bemoan the fact that an increase in aggregate output, which could have been deployed to the extra benefit of all, has gone begging. But there is nothing they can do about it so long as the parties cannot make binding long-run promises.

This model and its result depend on several assumptions we have made. When we deal with possible extensions, we shall proceed to change several of these assumptions in turn and examine how the result is affected. The stark result of a total lock-in ($\tau \approx 1$) is indeed special, but a partial lock-in, or a private incentive to move less than its social desirability, or $\tau > 0$, is a very robust feature of the situation. Therefore a general presumption that the process of redistributive politics will reduce economically efficient reallocation of resources seems valid.

THE TEXTILE AND APPAREL INDUSTRY

Virtually all of the industrialized countries make special provisions for their textile and clothing industries. This industry has benefited from exceptions to the General Agreement on Tariffs and Trade negotiated over a series of years, including the Short Term Arrangement on Cotton Textiles and Apparel in 1961, the Long Term Arrangement in 1962, the first Multi-Fiber Arrangement in 1974, with its successors in 1977, 1981, and 1986 (Cline 1990).

In the United States, textile and apparel workers have a number of *political* characteristics that are conducive to their being recipients of income transfers. Most importantly, these workers are politically central (large $\phi_i(0)$ in the swing voter model). The decline of textile manufacturing in the northeast and the industry's relocation to the southeast has contributed to the clout of textile workers. Southern Democrats are clustered about the ideological median of the U.S. Senate (Poole and Rosenthal 1991), and these senators are likely to count textile workers as part of their reelection constituencies. For leaders on both sides of the aisle, including clauses according favorable treatment to textile workers can help increase support for proposed legislation among pivotal moderate southern Democrats. Garment workers are also concentrated in politically pivotal states: (Cline 1990). California, Pennsylvania, New Jersey, and New York, a group of states crucial to success in *presidential* politics.

Next, education levels and union membership are also low among textile and apparel workers, weakening their attachment to political ideology and hence increasing their receptivity to offers of redistributive benefits for their industry (high κ_i).

Finally, workers in this industry tend to earn wages below the average for manufacturing. Therefore they have a high marginal utility of income, which tends to make them more responsive to offers of redistributive private consumption benefits.

In contrast, the *economic* characteristics of textile and apparel workers are considerably less auspicious. The world industry has been characterized by the rapid shift of comparative advantage of the labor-intensive clothing industry to developing countries with low per capita income levels. Technological progress has been slow in clothing manufacture, and while the productivity advantage of the United States is appreciable, it is insufficient to offset labor cost differentials of as much as fiftyfold (Cable and Baker 1983). In contrast, rapid technological progress has kept the U.S. textile industry nearly competitive with East Asian suppliers, though European textile manufacturers have fared much worse in this competition.

Both textiles and apparel receive substantial protection from foreign competition, with the textile industry receiving an effective tariff rate (including the tariff equivalent of import quotas) of between 30% and 50% imposing an estimated annual cost per job saved in the neighborhood of $135,000, while clothing prices receive an effective tariff of about 50%, corresponding to an annual cost per job saved of about $80,000 (Cline 1990, esp. p. 15; Hamilton 1984, 106). Cline (1990) argues that the annualized costs of the temporary unemployment that would follow removal of protection for textiles and apparel would be but a fraction of the ongoing costs per job saved.

Thus we return to our theme: Why are transfers—in this case to textile and garment workers—made by way of the economically inefficient conduit of trade protection, instead of by some more direct means, such as lump sum cash transfers that would not reward workers and investors for continuing their participation these industries?

THE LOGIC OF LOCK-IN

Given that the apparel industry has various political characteristics that make industry participants attractive as recipients of redistributive transfers, let us consider the decision of a particular garment worker whether to remain in the industry or to accept a job elsewhere (e.g., in retail trade). If she moves to retail trade, many of her political characteristics will remain essentially unchanged. For example, her attachment to politics (as defined in terms of the parameter κ_i) is unlikely to strengthen, while her income will probably remain low—both factors continuing to make her an attractive candidate for transfers. Other traits may change, though the direction of change is hard to predict a priori. For example, she may go to work for a department store that has been unionized. Union membership might be expected to move her politics to the left, perhaps far enough to make her less likely to change her vote in response to transfers and so less likely to receive favorable treatment. It may be that her job change will entail a geographic move (though in the case of a low-wage textile worker with family responsibilities this seems less likely) and hence a new position in terms of the political importance of her state.

While we might make various conjectures about whether the worker's political characteristics will change in response to the move, our neutral assumption was that they will remain unchanged. Surely if the job change does not entail either a geographic move or a major change in earnings her voting decision is likely to remain about equally receptive to offers of income transfers. We shall illustrate the operation of this neutral assumption and then examine consequences of relaxing it.

The worker foresees that much of the gain to total output from her relocating would be allocated to others through the political process, while she would bear many of the costs of relocation, in the form of having to learn new skills, establish new workplace relationships, adapt to a new schedule, and so forth. If taking the new job implies geographic relocation, the costs of moving will rise even higher, because the worker must resettle her entire household. Even if the worker's increased earnings would be sufficient to compensate these costs she may balk if the calculations highlighted in expression 13 reveal that in the political equilibrium she will be unable to keep enough of the benefits of the move to justify the dislocation costs.

In our stylized model, the effective tax rate was nearly 100%. In practice, deadweight losses from the available tax instruments drive governments to apply tax rates well below 100%. Nevertheless, the textile worker will be *partially* locked in. She will accurately foresee that if she moves her political clout will remain the same, and hence the change in her redistributive spoils as a result of the move will be negligible. She will weigh only the fraction of her income that escapes redistributive taxation against the costs of moving and so will remain in place until the social benefits outweigh *both* the costs of relocation *and* her personal tax burden.

As indicated in our analysis of the preceding section, the political parties do not *want* to produce these inefficiencies. But commitments to let workers who move keep more of their incomes will be broken under the competitive pressure of the next election, and everyone anticipates this. For workers with low clout, promised "relocation assistance" will fail to materialize, while high-clout workers who remain in place will continue to receive transfers, perhaps relabeled as "protection from unfair foreign competition." In the case of textiles, protection remained in place even when, as in 1980, the United States ran a slight trade *surplus* (Cline 1990)!

In practice, the political benefits to textile and apparel workers take the form of tariffs and quotas that protect the industry from import competition. These have a twofold effect: they encourage resources to remain in this industry (a misallocation on the production side) and raise the prices to consumers (a distortion on the consumption side). Each effect entails some economic cost, but in principle the two effects could be separated, and the industry could be subsidized to keep workers employed there, while allowing consumers to buy at lower prices. The subsidy will have to be financed using revenues from income or sales taxes (which involve their own costs to consumers), but it is found in practice that the cost to consumers of tariffs and quotas is substantially higher. Our model explains why the production-side cost emerges in the form of lock-in as part of a dynamic equilibrium of redistributive politics but does not address the question why import restrictions are used for this purpose, despite their extra cost to consumers. On this point, some combination of the dynamic inconsistency of our model, the misinformation and free-riding of the client politics model, and the lower diplomatic costs of negotiating "voluntary export restraints" with suppliers relative to imposing tariffs may provide greater insight than does any one model alone.

The textile and apparel illustration leads naturally to extensions of the basic model, and to these our attention now turns.

EXTENSIONS OF THE MODEL

We developed the model in a minimalist fashion to focus on the one aspect that is of crucial importance to the result, namely, that the politicians cannot credibly commit themselves to the full stream of future transfer payments. The other assumptions can all be relaxed and still yield some inefficiency in the resource allocation, although the changes do affect the precise form in which the inefficiency manifests itself. We now examine some such extensions and generalizations of the model. Since the ideas are generally quite evident once stated, we do not rebuild the whole mathematical apparatus in greater generality.

More Complex Dynamics

Perhaps the most important addition to the model would be to allow some commitment of future policies. Thus there might be one round of voting first, followed by occupational choice decisions. The party that emerges victorious in this round must implement its stated policies. Now in this round the usual argument for efficiency will apply: a configuration that produces larger output, along with an appropriate distribution of its fruits, will be able to command unanimous support over an inefficient alternative. If this were all, then the occupational choice decisions would also be the economically efficient ones. But so long as there is another round of voting after the migration choices have been committed, some inefficiency will reemerge. Each party will have an incentive to levy taxes and transfers in the manner we have examined, thereby taxing away at least some of the gains of the movers.

Movers Not Random Sample of Group

We maintained the "neutral" assumption that the movers are a random sample or a representative subgroup of the whole group of workers in the declining industry. This meant that the subgroups of movers and stayers had the same voting probability functions P_m and P_s and therefore ended up with the same consumption quantities. But in practice movers can be a biased sample. In principle, the bias can go any way, but we can suggest some possibilities that seem more pertinent in reality.

The answers depend on which of the two views of distributive politics we outlined is deemed relevant.

In the model of Lindbeck and Weibull (1987), both parties chase after the ideologically central marginal voters, while in that of Cox and McCubbins (1986), each party promises transfers to its core supporters; Dixit and Londregan (1995) give an integrated treatment of these approaches.

Begin with the Lindbeck-Weibull view. First suppose that workers in the industry to which those in industry 1 are contemplating moving are ideologically toward one extreme or the other. Then potential migrants from industry 1 who have similar ideological sympathies will find the migration relatively more attractive because they will find a ready ideological affinity with their new coworkers. This will make the movers' subgroup a nonrandom sample, emphasizing people closer to an ideological extreme. But that will bring them less by the way of transfers from the political process. Therefore the incentive to migrate will be reduced. The lock-in predicted by our simplest model will be reinforced. If, on the other hand, workers in the destination industry are predominantly centrists and therefore attract similarly inclined people from industry 1, then the movers subgroup *m* will be ideologically central and will receive better treatment from the politicians. This will restore some of the incentive to move.

Now consider the Cox-McCubbins view. Those people in group 1 who are core supporters of the winning party will have an incentive to distinguish themselves from the rest. If they can do so by migrating, that will restore the incentive to move. Offsetting this is the problem that transfers in reality are often mediated through some local politics or administration. Then the movers, being newcomers, are not going to be the core group in the destination location, so they may get poorly treated. The balance of these effects is unclear.

Yet a third perspective recognizes that individuals' political affinities for political parties and issue positions evolve over time and are influenced by peers and occupational status. Thus an auto worker moving from Michigan to Texas might be expected to develop a more "Texan" outlook, thereby changing political centrality. Changes in union status might similarly shift an individual's party affinities. If political and economic characteristics are not strongly correlated, individuals in industries receiving very favorable treatment in redistributive politics may anticipate that after relocating they will gradually acquire the more "average" political characteristics of their new location and hence more average treatment in the game of redistributive politics. This would further reduce the incentives for individuals in subsidized industries to move and it could *create* incentives to relocate for individuals in industries receiving unfavorable political treatment!

To sum up, the recognition of heterogeneity and nonneutrality among the workers in the declining industry does not result in any clear predictions that will overturn the basic result of inefficient lock-in. Under some circumstances, we should expect some partial mobility, but we should also expect migration to remain inefficiently small.

Consumption Not Fully Equalized

We assumed in our basic model that people care only about their disposable income—and not separately about their earned income and transfer receipts. As a result, we found that the political process gives them disposable incomes in relation to their political attributes, irrespective of the earned incomes. This is tantamount to a 100% marginal tax on incomes above the politically determined consumption level, along with a 100% offset when the earned income drops below this level.

This can be modified in two ways. In one, we can make utilities functions of consumption, as well as earned income $U(c, y)$, where the second argument enters negatively to reflect disutility of work.[8] Alternatively, we can make utility a function of a weighted sum $(y + \beta t)$, where $\beta > 1$ to capture the fact that people regard their own earnings as somewhat of an entitlement. Losing a dollar to taxes is worse than having a dollar less in earnings, and receiving a free dollar in transfer is sweeter than earning one.

Taking either of these approaches, we will find that the political process implicitly levies a tax that is positive but less than 100%. In the second approach, for example, the voting equilibrium will determine $(y + \beta t)$ for each group, so raising your earnings by a dollar will lose you $1/\beta$ in transfers. Then the incentive to move will be dampened but not totally eliminated.

More General Utility Function

Two features of our specification of moving costs in our basic model played a role in generating the complete lock-in: (1) they are incurred before the electoral process determines the redistribution, and (2) they are expressed in utility units and are subtracted from the utility of postvoting consumption. The first is an essential feature of our theme of dynamic inconsistency in the political process, but the second can be modified in a way that reduces the extent of the lock-in.

Suppose the individuals' utility function is of the form $U(c_b, c_a)$, where c_b is consumption before in the period labeled b—before moving and voting—and c_a is consumption in the period labeled a, after these two activities. Thus, the cost of moving comes out of c_b, while the political process yields taxes or transfers that alter c_a.

Our specification amounts to assuming a separable form, such as $U^b(c_b) + U^a(c_a)$. Then changes in c_b do not alter the marginal utility of c_a. Therefore a person's past moving decisions alter neither the future voting decisions nor the transfers he or she will receive in the political equilibrium (except for the insignificant effect of size $1/N$ already discussed).

Now let us suppose that c_b and c_a are imperfect substitutes in utility, so a reduction in c_b raises the marginal utility of c_a. Then those who have paid moving costs in period b will have a higher marginal utility of c_a. This will raise their responsiveness to transfers, so in the electoral game they will get more transfers. But the effect will fall short of compensating them dollar for dollar for their moving costs, because the two consumption quantities c_b and c_a are not perfect substitutes. Thus the implicit tax rate on the gains of a mover will be less than 100%. The incentive to move will be dampened, but not to the vanishing point as was the case in our main model.

CONCLUSION

In modern democracies, owners of resources in declining industries are often protected or subsidized in situ, instead of being encouraged to move to more productive uses. We have suggested that a dynamic inconsistency of the political process provides an explanation of this phenomenon. A promise of the kind "You will be able to keep enough of your gains to make the move worth your while" is not credible. Once people have moved, the political process will be free to renege on such promises and produce a new scheme of taxes and transfers that meets the political objectives of that time. In the standard model of redistributive politics, we see that in the voting equilibrium these taxes and transfers are governed by the political attributes of the various groups, and are essentially independent of considerations of economic efficiency. People will look ahead to this outcome and will not find it worth their while to incur the cost of moving. In a simple model, we find a total lock-in where no one moves; more generally, we expect the movement to stop short of what is required for economic efficiency.

Our approach also offers a useful perspective on some other commonly observed economic inefficiencies, for example the persistence of farmers cultivating areas subject to frequent flooding and homeowners in areas under threat of hurricanes or earthquakes. The transfers they will get if the risks come to pass depend on their group political characteristics, not on their economic decisions. This removes their incentives to make these decisions efficiently. Of course we all understand that the motivation for this behavior is "Why move or buy private insurance when the government will help us anyway?" What our analysis adds is the reason why the government will help anyway, namely the insight that the government's help is determined by political attributes that are distinct from economic choices.

Notes

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1. See Shepsle 1991 for a recent general survey of models of electoral competition.

2. We assume this to be constant and given, but a generalization that allows y_i to depend on the employment in the industry yields similar results.

3. This can be generalized to allow for a nonzero total budget available for redistribution, but that neither adds much to the analysis nor changes the results.

4. A Nash equilibrium in pure strategies exists if each party's payoff is (1) a jointly continuous function of both parties' strategies and (2) a quasi-concave function of its own strategy. For a recent exposition of this theorem, see Fudenberg and Tirole 1991, 34. Cox and McCubbins (1986) assume these conditions; we do likewise. See Dixit and Londregan 1995 for a more detailed discussion.

5. As usual, this multiplier is the implicit or shadow cost of party *L*'s budget constraint, namely how many more votes it could get if it was given an extra free dollar to spend.

6. Note that $\partial P_i/\partial t_{iL} = (\partial P_i/\partial c_{iL}) \times (\partial c_{iL}/\partial t_{iL}) = (\partial P_i/\partial c_{iL}) \times 1 = P_{iL}(c_{iL}, c_{iR})$.

7. If instead we modeled each individual as constituting an infinitesimal fraction of the population, the lock-in effect would be even *more* pronounced. Each individual's share of the extra output would be zero, the implicit tax rate τ would *equal* 1, and there would be no incentive to move.

8. We have omitted various labels *i*, *j*, etc. for simplicity.

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THE POLITICS OF PROPERTY RIGHTS

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The study of land tenure polarizes the field of development. Neoclassical scholars lobby for a move toward private property rights, while other economists and historians defend the maintenance of customary land tenure. I argue that the development scholars' focus on the structure of property rights obscures a more fundamental problem of land reform—that of enforcement. Property rights will not inspire individual investment and economic growth unless political institutions give the ruler of a local community or nation-state sufficient coercive authority to silence those who advocate an alternative, more distributionally favorable property rights system. At the same time, political institutions must force the ruler to establish a credible commitment to that property rights system. I illustrate this theoretical argument through an analysis of property rights institutions in Akyem Abuakwa, a traditional state in colonial Ghana.

Development scholars typically frame the debate over land tenure in polemic terms. Neoclassical economists insist that customary forms of land tenure impede economic growth in Africa (Dorner 1972; Harrison 1987; World Bank 1974), while a second group of scholars asserts that customary land tenure promotes productive investment (Bassett and Crummey 1993; Boserup 1981; Bruce 1988). Proponents of each theory then advocate policies intended to create or preserve the "ideal" property rights system.

Typically, customary land laws vest land in a community, such as a clan or lineage. Individual occupants of the land do not have exclusive rights to the land, and they cannot freely dispose of the land through sale or inheritance. Neoclassical economists criticize customary land tenure for two reasons. First, custom discourages investment on the land, because the individual occupants cannot be sure of reaping the full profits from their investment. Second, custom prevents the emergence of a market for land, since transactions are confined to the traditional community.

Anthropologists and some economists challenge the neoclassical prescription. They contend that customary land tenure adapts to changing economic conditions. With the creation of a cash crop economy, custom evolves to grant individuals greater rights of use and transfer (Migot-Adholla et al. 1991). Customary tenure can, therefore, provide a foundation for productive investment within each community.

In this essay, I contend that development scholars' focus on the ideal structure of property rights obscures a more fundamental problem of land reform, that of enforcement. The enforcement of property rights is neither easy nor automatic. Enforcement depends upon the ability of an actor or group of actors to measure the value of an asset's attributes, claim certain attributes for their own use, and prevent others from capturing those same attributes (Barzel 1989; Bromley 1989). Consequently, property rights are most likely to be enforced when actors possess ample information about the asset, few individuals or groups press for an alternative definition of property rights, rights holders have ready access to adjudicatory institutions, and those institutions are backed by the coercive authority of the state.

In the industrialized democracies of the West, a dense network of market institutions, a well-defined legal code, and abundant adjudicatory forums allow for the effective enforcement of private property rights. Developing country governments are unlikely to possess a comparable set of institutions. Thus neoclassical scholars who insist on privatization must be cognizant that private property rights may not be enforceable in all settings.

Similarly, those scholars who defend customary land tenure must consider whether all local communities possess the institutions needed to enforce a stable property rights system. Throughout Africa, customary land tenure laws evolved when land values appreciated at the turn of the century. Individuals and groups competed to appropriate the newly valuable resource by "reinventing tradition," defining a version of customary land tenure favorable to themselves. The system of customary law that emerged promoted economic development only if individuals and groups ended the dispute and enforced a coherent property rights system.

I argue that the productive potential of any property rights system depends not only on the way in which rights are assigned, but also (and perhaps more importantly) on the degree to which those rights are enforced (Coase 1960). I illustrate both the importance and difficulty of enforcing property rights by examining property rights institutions in colonial Akyem Abuakwa, a traditional state in the Gold Coast Colony (now Ghana). Throughout the Gold Coast Colony, the increase in land values coincided with the imposition of British indirect rule. Within this constitutional environment, indigenous actors tried to appropriate the newly valued land by defending principles of customary land tenure.

In Akyem Abuakwa, the state's paramount chief promoted a version of custom that gave him extensive control over the state's land. This property rights system promoted economic development in the state for two reasons. First, the paramount chief wielded sufficient coercive authority to defeat those who advocated an alternative definition of custom. Second, the paramount chief reformed the state's political institutions, establishing a credible commitment to uphold a single property rights system and giving all of the state's citizens a positive stake in that system. The key to the state's economic success, therefore, lay not in the structure of property rights, but in the chief's capacity to enforce property rights through the state's political institutions.

My analysis of the Akyem case proceeds in five stages. First, I describe Akyem Abuakwa and discuss the nature of customary land tenure prior to the imposition of British indirect rule. I then outline the changes wrought by indirect rule and by the state's integration into a capitalist economy. Second, I present the indigenous actors' response to the commercialization of land, specifying the type of property rights system that they defended. Third, I examine the paramount chief's effort to enforce his preferred property rights system. Finally, I offer a partial test of my argument by examining the revenue and expenditure patterns of the Akyem Abuakwa State Treasury.

AKYEM ABUAKWA AND THE COMMERCIALIZATION OF LAND

The state of Akyem Abuakwa covers three thousand square miles and was in 1921 the sixth most populous state in the Gold Coast Colony (Danquah 1928, 47). Sixty miles inland from Accra, it lies in the heart of Ghana's forest belt. The humid climate and the fertile soil make the land ideal for the cultivation of cocoa, while the natural forests provide ample timber for commercial use. In addition, portions of the state, primarily within the Birim River valley, contain rich diamond and bauxite deposits.

The Akyem people are members of the Akan linguistic group, and the state's political organization mirrors that of other Akan states. The rural population is divided into multiple villages, each governed by a family head. Villages are then grouped into towns, governed by an *odikro*. Finally, the towns are grouped into a number of divisions—each governed by an *ohene*—and wings. Five of the divisional chiefs also serve as wing chiefs. All of these different units are subject to the authority of the paramount chief, the *okyenhene*. Each of these chiefs sits on a *stool*. Literally, that stool is the chief's throne. Figuratively, it is the embodiment of each division or state.

Prior to the commercialization of land, the divisional chiefs in Akyem Abuakwa claimed ownership of the land on behalf of all stool subjects, past, present, and future (Asante 1975; Hill 1963a; Woodman n.d.). The contemporary stool holder allocated plots of land to citizens of the stool, who then exercised rights of usufruct. Upon a citizen's death, the land and any improvements made upon the land reverted to the stool. In addition, the chiefs themselves retained a portion of the land for their own use.

Land values began to appreciate just before the turn of the century, when the presence of large European trading houses and improvements to the Colony's road-and-rail system combined to expand the market for cocoa and timber. Immigrant farmers entered the state in large numbers to claim the fertile Akyem lands for cocoa farming (Hill 1963a, 138–45; idem 1963b). European concessionaires hastened to claim mineral and timber rights in the region. Divisional and town chiefs alike responded to the growing demand for land by selling stool lands outright. At that time, customary law made no distinction between stool property and individual property, leaving the chiefs free to claim the proceeds from land sales for their private use.

Citizens of the state witnessed the loss of the state's patrimony—their own farm land—and moved to sanction the chiefs for their behavior. Commoner citizens turned to the traditional institution of the *asafo*, an organization of nonroyal males assigned to protect the villages in case of military attack. By 1917, the *asafo* companies in Begoro, Apinam, and Kwaben had destooled their respective *ohene* for the indiscriminate sale of land (Simensen 1975). They were not alone in their protests. Between 1904 and 1926, the Colonial Office recorded some 109 cases of destoolment in the Colony (Hailey 1957, 191).

Paradoxically, the wave of destoolments only heightened the pace of land sales in Akyem Abuakwa. As the threat of destoolment grew, each chief knew that his tenure in office would likely be short. He therefore had every incentive to sell the land as quickly as possible, maximizing his profits in the little time available to him (Levi 1988). Individual chiefs literally competed with one another to sell the land. By 1928, the divisional chiefs had sold an estimated 98% of the stool's land from the Densu River and Nsawam to Densusu, near the village of Apedwa, a strip 25 miles long and 10 to 25 miles in width (Danquah 1929, 31). Equally large portions had been sold to Krobo farmers in the northern portion of the state.

For individual citizens, the land rush was an economic disaster. The chiefs's competition to sell the land depressed prices,[1] a small number of citizens claimed the vast majority of the profits from the sales, and the majority of the state's citizens were simply disinherited. The sales systematically transferred Akyem land, and with it all future wealth, to stranger farmers and European concessionaires.

Nana Ofori Atta, the paramount chief of the state, abhorred the losses. For Ofori Atta, the land sales provided a constant reminder of the paramount stool's declining influence over the divisional stools. According to custom, the paramount chief was entitled to receive one-third of the rents from land transfers as tribute. When land values (and therefore rents) appreciated, the divisional chiefs ignored that custom. The paramount chief lacked sufficient political authority to enforce his rights.[2] Thus the sales both deprived the paramount stool (and the paramount chief personally) of substantial economic gain and undermined the power wielded by this centralized authority.

THE REINVENTION OF TRADITION

Nana Ofori Atta was enstooled as paramount chief of Akyem Abuakwa in 1912. He reigned until his death in 1943. J. B. Danquah, Nana Ofori Atta's half-brother and a prominent politician in his own right, served the state as legal advisor.[3]

Throughout their long careers, these two men worked in concert to make Akyem Abuakwa one of the most powerful states in the Gold Coast Colony and to enhance their own political and economic well-being. To achieve these goals, they sought to articulate and enforce a system of customary land tenure that would check the indiscriminate sale of land, channel resources to the paramount stool, and encourage the state's citizens to invest in the land.

Ofori Atta launched the campaign to redefine customary land tenure soon after his enstoolment. Nevertheless, property rights were not clearly defined until 1932, and they were not well enforced until approximately 1940. In an initial effort to define property rights, the Okyenhene's Council passed a bylaw stipulating that all land sales made by the divisional chiefs had to be preapproved by the Okyenhene. The divisional chiefs ignored the bylaw and continued to liquidate the stool's resources.

In 1929, the Okyenhene's council passed the Akyem Abuakwa Stool Lands Declaration. The bylaw defined *stool land*, in the broadest possible terms, as "any land in Akyem Abuakwa which is not sold to a non-native of Akyem Abuakwa," including that land currently farmed by Akyem citizens and family members. The law also affirmed the Okyenhene's right to approve all land sales and asserted the stool's right to collect one-third of the profits from the sale, in accordance with customary law (Akyem Abuakwa Stool Lands Declaration 1929).

The British governor rejected the bill in its original form, claiming that Ofori Atta had contravened customary law as he understood it. The governor's objections prompted a series of drafts and redrafts, which culminated in the passage and approval of the Akyem Abuakwa State Council Stool Lands Declaration of 1932. The revised version of the bylaw embodied a weakened definition of stool land. Stool land was said to include "land attached to the stool over which the stool exercises some measure of control." Ownership of stool lands was vested jointly in the paramount and divisional stools (rather than in the paramount stool exclusively). Significantly, however, the published version retained the paramount chief's right to approve all land sales.

Ofori Atta chose to defend principles of customary land tenure for two reasons. First, within the constitutional framework of indirect rule, private property rights were unenforceable. Under indirect rule, the British sought to minimize the cost of administration by delegating authority to Africa's traditional rulers. The British relied upon the chiefs both to provide them with the information that they needed to make policy decisions and to implement British decisions within the traditional states. The British dependence on the chiefs compelled colonial officers to take measures to bolster the chiefs's authority. The maintenance of customary land tenure was the most important of these measures (Phillips 1989). British officials, therefore, were extremely reluctant to grant private rights to individual citizens (Basset and Crummey 1993; Berry 1993; Migdal 1988). Such action would only undermine the structure of colonial rule.

Second, Ofori Atta defended customary land tenure because private property rights did not benefit him. Ofori Atta recognized that if he defended a system of private property rights, he risked losing the bulk of Akyem lands to wealthy European concessionaires and immigrants from neighboring states. The divisional chiefs would likely claim the profits from all sales, and the paramount stool would be left without a source of present or future income.

Ofori Atta chose to defend the customs articulated in the Stool Lands Declaration, because they promised to give the paramount stool and its occupant a larger portion of the state's income. Under the status quo, most of the revenue in the state flowed from the sale of land. Because the paramount stool could not enforce its right to a one-third tribute, it gained little. Under the new property rights system, most revenue would derive from productive investment on the land.

With the Stool Lands Declaration, each stool subject enjoyed full rights of usufruct to the land cultivated. As long as a citizen transacted with another stool subject, then he or she could sell land to other citizens, let the land to share tenants (called *abusa* farmers), or deed the land to a community member. If an individual built upon or improved the land, he or she would be the sole owner of the additions. The stool's right to reclaim the land applied only if the land was abandoned for a lengthy period. If Ofori Atta believed that his reign as paramount chief was secure, then he would have an incentive to commit himself to enforcing such a system; for over time, the stool could capture a portion of the increased revenue in the form of a tax, guaranteeing the state and the paramount chief a stable, expanding source of long-term income (Rathbone 1993, 59).

Ofori Atta battled to enforce the new property rights system on two fronts. First, he struggled to quell domestic opposition to the new system. Second, he worked to establish his own commitment to the new system, thereby convincing citizens that their rights were secure and encouraging citizens to invest on the basis of those rights. I examine each of these endeavors in turn.

ENDING DISTRIBUTIONAL CONFLICT: THE NEED FOR COERCIVE AUTHORITY

The successive Stool Lands Declarations promised to alter significantly the existing distribution of resources in the state. Under the status quo, the divisional and village chiefs claimed the profits from the sale of stool lands and openly defied the paramount stool. If Ofori Atta could enforce his proposed property rights system, the paramount chief could claim the divisional chiefs's wealth and power.

Ofori Atta's efforts to redefine customary land tenure did not go unchallenged. In 1921, the divisional chief of Asamankese issued a declaration of independence from the paramount stool of Akyem Abuakwa and denied Ofori Atta's claims over Asamankese land. The chief's actions were almost certainly prompted by the discovery of extensive diamond deposits in and around Akwatia, a village in the Asamankese division. The discovery made Asamankese the wealthiest division in Akyem Abuakwa and thus heightened the distributional consequences of the new property rights system.

The contestants' battle to define property rights became a battle to define tradition. The Ohene of Asamankese contended that in ancient times, the Asamankese and Akyem stools had allied to fight their mutual enemy, the Akwamu. Their relationship now was one of friendship and mutual convenience and had never involved any form of hierarchical authority such as that now asserted by Ofori Atta. Therefore the Okyenhene's efforts to assert control over Asamankese lands constituted a breach of native custom and was ample justification for Asamankese's secession.

In contrast, Ofori Atta claimed that the Akyem Abuakwa state had defeated the Akwamu in battle at a time when the Asamankese still served the Akwamu paramount stool. When the Akwamu were defeated, the Asamankese swore an oath of allegiance to the Akyem paramount stool. As part of this oath, the Asamankese promised to deliver one-third of all gold or treasure found on stool lands to the paramount stool (United Kingdom 1930).

The British magistrate who heard the dispute between the paramount stool and Asamankese accepted a portion of both parties' claims. The magistrate ruled that Asamankese was by custom part of the Akyem hierarchy, and that Ofori Atta could claim one-third of all revenue from the land. At the same time, however, he ruled that the divisional chiefs could still sell or lease land without the paramount stool's consent. The paramount chief need only be apprised of such transactions.

In spite of this decision, the Ohene of Asamankese continued to press his claims, appealing the magistrate's decision through every legal channel available to him. Throughout the protracted legal battle, the Ohene continuously challenged the paramount chief's authority and undermined all efforts to enforce a new property rights system.

The Ohene's and Okyenhene's battle to redefine tradition was common to all of the traditional states in the Gold Coast Colony. Even before British colonization, indigenous Africans contested the definition of custom to gain access to and control over resources. With colonization, that contestation only intensified as indigenous actors, working within the framework of indirect rule, pursued new economic and political opportunities through the institutions of the traditional state (Berry 1993, 25–27).

Colonial officials viewed the competition to reinvent tradition with trepidation. They knew that each version of custom represented a demand for social and economic change; each custom could, therefore, provoke widespread civil unrest, straining British resources and disrupting trade and commerce. But colonial officials had no way to evaluate or compare the competing claims to discern which were most dangerous and which most benign. Colonial institutions, built around the assumption that custom was fixed, did not provide them with the necessary information.

In the absence of such information, the British adopted a cautious, hands-off approach to disputes over custom (ibid., 38). They refused to intervene consistently on behalf on one claimant, insisting instead that all controversy must be settled at the local level "by traditional means" (Sutton 1984).

In most cases, the British stance meant that disputes over custom went unresolved. Typically, no claimant wielded sufficient coercive authority to defeat their opponent outright, forcing them to accept a disadvantageous outcome; and no party could persuade the British to intervene on their behalf (Firmin-Sellers n.d., chap. 3). Thus, within most states, the competition among groups to control the office of the paramount chief continued; both property rights and custom remained insecure and fluid.

In Akyem Abuakwa, however, the dispute over custom was resolved. Ofori Atta exploited his unique position in the colonial state to capture British coercive authority and halt conflict over the definition of property rights.

Linking the Community to the State

Under indirect rule, the British relied upon the traditional rulers to implement British policy in their states and to serve as advisors during policymaking. Ofori Atta distinguished himself in this latter role. He held a prominent position in the Gold Coast Legislative Council, where he served from 1916 until his death in 1943. Colonial officials hailed Nana Ofori Atta as a "great statesman" and regarded him as the "unofficial advisor on native affairs" (Wight 1948, 69–70).

Ofori Atta used this privileged position to advance his political agenda. Ofori Atta persuaded British officials to redesign the institutions of indirect rule so as to consolidate his own authority over other paramount chiefs in the colony. With each institutional innovation, Ofori Atta gained greater influence over British policymaking. Increasingly, Ofori Atta was able to persuade British officials to take policy decisions that advanced his own, private concerns. Specifically, he convinced British officials to take action against the recalcitrant divisional chiefs in his state. In this way, Ofori Atta captured British coercive authority and ended distributional conflict over property rights in Akyem Abuakwa.

Consider, for example, Ofori Atta's actions in the provincial councils, first created in 1926. Originally, the provincial council system divided the chiefs into three distinct provinces: East, West, and Central. The Eastern Province was subdivided into three linguistic groups: Ewe, Fanti, and Twi. Each province deliberated and voted separately; and within the Eastern Provincial Council, each linguistic group voted separately. Under this arrangement, a chief in one province or linguistic group could not influence the votes of chiefs in other provinces, and the chiefs themselves could not speak as a block.

Ofori Atta campaigned to eliminate the barriers between the three provincial councils. In April 1932, he coordinated a joint meeting of the three provincial councils; a second joint session was held in 1934. That same year, he petitioned the British to grant the Joint Provincial Council statutory recognition. When the request was denied, he appointed a committee to explore alternative means by which to unite the three provincial councils (Frimpong 1966, 37). It is not certain what the committee of enquiry recommended, but the joint sessions resumed in 1936, were granted statutory recognition in 1946, and continued uninterrupted until both the provincial councils and the Joint Provincial Council were abolished in 1958.

The Joint Provincial Council quickly became a dominant force within the colony. At Ofori Atta's instigation, the Joint Provincial Council arrogated to itself the judicial, advisory, and appointive powers of the provincial councils (ibid., 37–42). In addition, the Joint Provincial Council began to mediate all contact between the chiefs and the British officers.

Significantly, Nana Ofori Atta stood at the nexus of this contact. As president of the Joint Provincial Council and chair of its standing committee, Ofori Atta had gained exclusive authority to speak for all of the colony chiefs. Administrators recognized his monopoly power, noting that "it is quite obvious today that [Ofori Atta] is attempting to direct the policy of the chiefs throughout the Colony" (Secretary for Native Affairs 1941).

Through the redesign of colonial institutions, Ofori Atta gained ever greater control over the flow of information from traditional rulers to colonial authorities. However, Ofori Atta had no incentive to reveal that information accurately. The design of the Gold Coast Legislative Council denied Ofori Atta (or any African representative) the opportunity to cast a meaningful vote, restricted his authority to write legislation, and allowed the colonial governor to amend his policy proposals at will. Under these circumstances, Ofori Atta could most effectively influence legislation by distorting his privately held information, using it to persuade British officials to write general policy that addressed his particular concerns (Firmin-Sellers n.d., chap. 2; Gilligan and Krehbiel 1989).

Ofori Atta's growing ability to influence British policy making is vividly illustrated by his role in the drafting and subsequent amendment of the Native Jurisdiction Ordinance of 1927. In 1925, the governor of the Gold Coast Colony invited the chiefs to submit a proposal to amend the Native Jurisdiction Ordinance of 1878. Ofori Atta convened a series of meetings with both chiefs and educated men of the Eastern Province to draft the new legislation.

Ostensibly, the draft addressed general concerns. But, many of the bill's provisions aimed specifically to quash the Asamankese rebellion. The ordinance made it unlawful for "any Head Chief, Chief or Headman within a state to disclaim dependence to a Paramount Stool . . . [or] on any pretext whatsoever transfer or withdraw his or her national allegiance from the Paramount Stool or from the sub-stool" (Memorandum and Draft Native Jurisdiction Bill 1925). Further, any dispute over the relationship between paramount and head chief was to be decided in the Oman Council, whose decision would be subject to appeal to the provincial council and the governor. A 1929 amendment (made at Ofori Atta's behest) gave the paramount chief the power to punish individuals who instigated opposition to a paramount or divisional chief, and, with the governor's approval, to deport that individual from the state (1929 Amendments to Native Administration Ordinance 1929).

Ofori Atta consistently argued that the Native Administration Ordinance and its subsequent amendments were essential for all states in the Colony, not just Akyem Abuakwa. In addition to these measures, however, Ofori Atta appealed to the British to pass measures which directly addressed the dispute in Asamankese. In these instances, he framed his requests in terms of British interests. In 1930, for example, Ofori Atta sent a memorandum to the district commissioner requesting that the colonial government force the Asamankese stool to "render due services to the Paramount Stool." He warned the British that their failure to act would demonstrate that "the British government is really powerless to interfere, . . . that any chief would defy his paramount stool without the government being in the least concerned as to the consequences. . . . It is an open secret that the manner in which the government treats the Asamankese affair will more or less determine the fate of other states in this country" (Ofori Atta 1930).

Colonial officials eventually came to accept the logic. In 1933, the governor of the colony warned the secretary of state, Cuniffe-Lister, that "the Asamangkese case is becoming regarded as a test of the ability of the Government to assert itself when justifiable grounds exist and of its *bona fides* in proclaiming itself to be the protector of the people as their trustee against oppression and injustice" (Thomas 1933). Shortly thereafter, the colonial government passed the Peace Preservation Ordinance to help quell the persistent unrest in Asamankese. And in 1935, the government approved the Asamankese Division Regulation Ordinance, providing for the "control and regulation of property, revenues, and expenditures of the stools in the Asamangkese Division."

The British support for Ofori Atta was not confined to the Asamankese case. Indeed, Rathbone contends that Ofori Atta "was supported by . . . the power of the colonial state whenever he needed it" (1993, 55). In 1918, for example, representatives of the *amantoomiensa* (the chiefs of the five towns surrounding Kyebi) marched on the state's capital to protest Ofori Atta's rule.[4] British troops physically blocked the protestors' entry to the capital, and individual protesters were later singled out for "severe punishment" by the British district commissioner. And in 1932, when several *asafo* companies launched destoolment proceedings against Ofori Atta, British officials repeatedly affirmed their support for the Okyenhene (idem 1989, 447–8).

Ofori Atta was criticized by his contemporaries and by historians for being a collaborator, a crony of British imperialism who used his position to enhance his own wealth. While the accusations may have merit, they nevertheless obscure an important point: Ofori Atta prospered because he manipulated his ties to the colonial state and persuaded the colonial government to use its coercive authority to end conflict over the definition of customary land tenure in his state.

The nature of Ofori Atta's success, however, underscores the weakness of British indirect rule. The British delegated authority to the chiefs, because they believed that the traditional rulers could maintain law and order in their states (a prerequisite for trade and production) at the lowest cost. That decision made it imperative for the British continually to reinforce the chiefs's power. To this end, the colonial government intervened in Akyem Abuakwa. They did so because Ofori Atta had convinced them that their inaction would undermine traditional authority everywhere in the Gold Coast Colony.

Ofori Atta's strategy was not replicable. The institutions of indirect rule gave Gold Coast Africans few points of access to colonial officials, and Ofori Atta monopolized most of these. The paramount chiefs from other states, therefore, were not in a position to influence British decision making through the manipulation of privately held information. Most paramount chiefs could not convince the British that ongoing disputes in their individual states would undermine traditional authority more generally; or that enforcement of their opponents' versions of custom would be destabilizing. Consequently, other traditional authorities could not convince the British to intervene on their behalf.

Ofori Atta's ties to the colonial state were pivotal. Ofori Atta was not the only politician in the Gold Coast to champion the reform of indigenous institutions. However, Ofori Atta was one of the few individuals who had both the capacity and the incentive to act upon his ideals. British support for Ofori Atta lent the chief the coercive authority he needed to defeat those who opposed his preferred property rights system. Equally important, British support offered the chief a rare safeguard against the threat of destoolment. Under these circumstances, Ofori Atta could adopt a longer-term perspective, in which the benefits of committing himself to the new property rights system outweighed the gains from periodic malfeasance.

THE CASE FOR CONTRACTARIAN REFORM

I have argued that Ofori Atta enforced property rights by capturing British coercive authority and ending conflict over the definition of those rights. Coercive force was a necessary but not sufficient condition for the creation of a secure property rights system, however. Coercion alone is insufficient, because a ruler powerful enough to end distributional conflict and enforce property rights is also powerful enough to violate those rights and confiscate his subjects' wealth. Therefore, property rights will not be secure until the ruler establishes a credible commitment not to abuse his or her power (North and Weingast 1989; Weingast 1993).

In a state, both ruler and ruled may benefit if the ruler tries to enforce a single property rights system.[5] When the ruler upholds property rights, citizens are willing to invest, knowing that they will receive the profits from that investment. The ruler claims a portion of the subsequent economic growth in the form of taxation. Over time, both individual incomes and the ruler's tax receipts increase. Society as a whole prospers.

This welfare-enhancing outcome is elusive, because at any given point in time, the ruler has an immediate incentive to violate property rights and confiscate all of his or her subjects' wealth (rather than the small fraction gained through taxation). If the ruler succumbs to this short-term temptation, however, citizens will no longer invest. Instead, citizens will hide their assets or flee the ruler's domain. Economic growth grinds to a halt and, in time, both ruler and ruled suffer. To avert such an outcome, the ruler must find a way to commit him or herself to the pursuit of long-term gains and the maintenance of a single property rights system.

New institutionalist scholars have begun to analyze the mechanisms by which rulers establish a credible commitment to behave cooperatively and pursue a collectively beneficial outcome. Such mechanisms take three forms. First, rulers can give their subjects a positive incentive to abide by their mandates. For example, the ruler can encourage subjects to pay their taxes voluntarily by making the subjects residual claimants to public revenue. The ruler accomplishes this by exchanging taxes for public goods provision (Levi 1988, 54). Second, rulers can use their position to signal to the subjects which cooperative strategy the subjects should adopt and to enhance the subjects' expectations that all others are cooperating (Calvert 1991; Miller 1992, 196). Third, and most importantly, rulers can use their positions to signal an intention to behave cooperatively, establishing a credible commitment to abstain from pursuing their immediate self-interest. To this end, the ruler may create formal channels for two-way communication, establishing the ruler's interest in listening to the subjects' preferences and suggestions. Along similar lines, the ruler may institutionalize joint decision-making procedures over a range of issues, establishing a precedent for cooperation. The ruler may forgo privileges to create an atmosphere of equality. Finally, the ruler may agree to impose institutional constraints on the ruler's own power, literally "tying the king's hands" (Root 1989). In this way, the ruler persuades the subjects that the ruler will not use power capriciously.

I argue that this theoretical notion of "credible commitment" was central to Ofori Atta's efforts to secure a stable property rights system in Akyem Abuakwa. Ofori Atta appealed to British coercive authority to end distributional conflict. At the same time, however, Ofori Atta worked with J. B. Danquah and other reformers in the state to (1) give all of the citizens of the state, including the divisional chiefs, a positive stake in the new property rights system; and (2) to establish a credible commitment to use his authority to uphold that property rights system.

Compensating the Loser

Nana Ofori Atta launched a concerted effort to give all citizens of the stool a positive stake in the new system. Consider the reformers' overtures to the divisional chiefs, those most disadvantaged by the new system of property rights. Under the status quo, the divisional chiefs earned substantial income from the sale of stool lands. Because destoolments were increasingly common, the divisional chiefs had every incentive to sell those lands as quickly as possible, maximizing the revenue earned during their short tenure in office.

Ofori Atta and Danquah sought to alter these incentives. First, they promised each divisional chief a regular maintenance allowance, a fixed guaranteed salary throughout the chief's tenure. Second, they fought to gain control over destoolment procedures, to ensure that those chiefs who upheld the new property rights system would serve for an extended time.

In 1925, for example, Nana Ofori Atta helped to organize a conference of paramount chiefs to discuss proposed reforms to colony administration. The group submitted to the Colonial Office a draft version of the new Native Administration Ordinance. They called upon the British to grant statutory recognition to the state councils (the bodies that arbitrated stool disputes) and to authorize the creation of new provincial councils, political institutions charged with helping states that found themselves unable to "bring to a happy close a dispute or controversy" (Memorandum and Draft Native Jurisdiction Bill 1925). The reforms would strengthen the paramount chief's ability to decide which of his subordinate chiefs merited destoolment.

Ofori Atta also pushed for legislation codifying the customary law of destoolment. Such codification would effectively eliminate the imprecisions in the customary law of destoolment that rivals within the state exploited to destool chiefs.

The reforms that Ofori Atta advocated promised to give the divisional chiefs an incentive to uphold the newly defined property rights system. The maintenance allowance would give the chiefs a steady source of income. The increase in the paramount chief's influence over destoolment proceedings would allow him to link that income to the chiefs's management of stool lands; the codification of destoolment procedures would extend the chiefs's time horizon so that the promise of a steady income would become more attractive than the lure of short-term profits from the sale of land.

Securing the Citizenry's Support

Ofori Atta and Danquah also sought to secure individual citizens' support by giving each a long-term interest in the enforcement of the new property rights system. They accomplished this by helping to create a treasury system in which the chiefs were made accountable to the citizens for their management of public funds and the citizens were made the residual claimants to profits from the chiefs's management of public resources.

The institution Ofori Atta and Danquah envisioned would eliminate the direct benefits the chiefs accrued from the sale of land by making them accountable for all monies received. Under a 1928 bylaw, the Okyenhene's council mandated that a state treasurer list all sources of revenue to which the state was entitled, including profits from the sale of land. The monies accrued from this state property would be deposited directly into a bank account, held in the state's name. Payments from the account could be used only for matters of "public welfare," as defined by a finance committee (State Finance and State Treasury Proposal 1928).

The bylaw is significant in that for the first time, it provided the state's citizens with the information necessary to draw a formal distinction between public and private property. It therefore became possible for the citizens to detect a chief's misappropriation of public resources and, through the state council and the custom of destoolment, sanction him for his actions. Citizens had an incentive to incur the costs of monitoring the chiefs because the bylaw made them residual claimants to profits from the management of stool funds. If the stool's resources were well managed, then the flow of monies into the treasury, as well as the expenditures on public welfare, would increase.

The Stool Lands Declaration and the State Treasury By-Law defined a new property rights system and gave the majority of the state's citizens—chief and commoner alike—a long-term interest in the system's success. Nevertheless, throughout the 1930s, the treasury never operated as intended. Few branch treasuries were created, and of those formally established, few opened. In those that opened, revenue collection was difficult. What accounts for the early failure?

The Problem of Credible Commitment

The state treasury was not an immediate success because the paramount chief, Nana Ofori Atta, had not established a credible commitment to uphold the new property rights system. Citizens would not invest in the land or pay their taxes to the treasury without some assurance that the paramount chief would not divert these public resources to his own private ends (Weingast 1993).

Danquah and Ofori Atta recognized this dilemma. Throughout the 1930s, they agitated for a series of reforms which would serve to establish the paramount chief's commitment, "perfecting the Leviathan" who wielded authority over the state's citizens. Their efforts took two forms. First, they created institutions of joint decision making. Second, they limited the paramount chief's authority by delegating power over a significant range of issues to nonchiefs.

Ofori Atta sought to generate cooperation by opening the Okyeman Council—the state's supreme deliberative body—to nonchiefs. In forging these institutions of joint decision making, a sovereign creates a forum for repeated face-to-face interaction between ruler and ruled. Both parties gain more and better information about one another's preferences and strategy choices as they engage in informal and formal discussion.

Danquah engineered the drive to open the Okyeman Council. By nineteenth-century custom, the Okyeman Council was composed of the royal stools in Akyem Abuakwa: the paramount stool (Okyenhene); the stools belonging to the five wing chiefs (Adontenhene, Nifahene, Benkumhene, Gyaasehene, Oseawuhene); the stools of the divisional chiefs who served under the wing chiefs (Otweresohene, Asamankesehene, Akyeasehene); and the stools of all of the village chiefs within each division. A retinue of nonvoting linguists and councilors accompanied each chief. The group served as advisors to the chief, or, if the chief was absent, one served as a delegated representative. The Okyenhene presided over the council, but he was not the absolute ruler. In theory, all decisions were made by the Okyenhene in council; in practice, power was concentrated primarily in the Okyenhene and the five wing chiefs (Addo-Fening 1980, 44–46).

Although the hierarchical structure of the council guaranteed representation to each geographical area, Danquah argued that some constituents (i.e., educated commoners) merited special representation on the council. If commoners sat on the council, they could work directly with the chiefs to further public welfare. To promote his cause, Danquah had formed the Akyem Abuakwa Scholar's Union in 1916 "[to bring] together all the educated sons of the state to offer leadership in a common endeavor to promote peace, unity and progress in the State" (ibid., 305).

The debacle that followed the 1931 Native Revenue Ordinance convinced the chiefs on the Okyeman Council of the need to open its membership to representatives of the educated community. Ofori Atta's support for the bill had sparked widespread protest within the state, and two wings had threatened to withdraw their allegiance from Ofori Atta. At this juncture, the Scholar's Union stepped in to mediate and persuaded the wings to travel to Kyebi and negotiate their differences. In return for their service to the state, the Okyeman Council granted the union three seats on the state council.

Once convinced of the need to include the union, Ofori Atta moved to institutionalize their representation and to open the council to other key interests. In 1939, Ofori Atta introduced a measure to the Eastern Provincial Council to include "enlightened opinion" on all native councils (Political Conference to Secretary for Native Affairs 1939). In 1944, the state council agreed to open its membership to stranger farmers (Report of Estimates Committee for 1944–45, 1944). And in 1945, Ofori Atta's successor, Ofori Atta II, endorsed Danquah's recommendation that the Okyenhene's council expand its membership to include the state secretary, the principal of Abuakwa State College, and 10 members of the Kyebi Executive Council (Ofori Atta II 1945).

These reforms did not wrest control of the Okyeman Council away from the traditional authorities. In 1944 the Akyem Abuakwa State Council had 40 members. Eight represented special constituencies and the remainder were chiefs or their functionnaires—an unusually large number of commoners relative to other states (Hailey 1957, 206). The 1945 additions notwithstanding, the balance of power still lay with the chiefs. The value of the reforms, therefore, lay in the creation of a forum for repeated, face-to-face interaction between the chiefs and representatives of the citizens. Within this environment, they learned reliable information about others' preferences and strategies. Danquah hoped that through the exchange of information, the educated commoner would expose the chief to a new set of feasible policies which would serve both the chief and the state.

The second measure adopted to perfect the Leviathan involved the delegation of authority. The chiefs literally agreed to tie their own hands, limiting their authority over some specified jurisdiction (Root 1989). Substantively, the delegation of authority eliminated some of the chiefs' capacity to behave as unfaithful agents of the citizens and thereby lowered the risk an individual faced in deciding to invest or pay their taxes to the treasury. Symbolically, the delegation signalled the chiefs' broader interest in preserving a long-term cooperative outcome. In both cases, individual expectations about public outcomes shifted to make cooperative outcomes more likely.

Danquah himself implicitly recognized the need to limit the chiefs's authority. Following the demise of the Native Revenue Ordinance, Danquah conceded that the measure may have been premature. An editorial in the *Times of West Africa* commented, "We have always held the view that until the people of this country, the people in every state, realize the urgent necessity of finding a method of keeping state funds in a systematic order, as well as safeguarding the security of such stool funds, the independence of the State Council will never become a fact." He added that the people would not, and perhaps should not, support this bid to become independent until the chiefs "prove themselves worthy of [holding power]. . . . Those who desire to rule, says Aristotle, must first submit to be ruled. The chiefs, too, must submit to discipline" (27 October 1933).

The Okyeman Council did agree to "submit to discipline" and impose institutional limitations on their authority. Specifically, the council decided to delegate the powers of oversight and management of the state treasury to a finance committee. As originally conceived in 1928, the finance committee consisted of the seven wings chiefs, eight divisional chiefs, five representatives of the Kyebi Executive Council, three chiefs from the Amantoomiensa, the state treasurer, and the Okyenhene (Passage of State Finance and State Treasury Proposals 1928). By 1937, however, the state council had agreed to a substantially different committee with a membership of only seven. Of the seven, four were educated representatives of the divisional chiefs, and a fifth was a delegate from the Scholar's Union. Only two chiefs were present. J. B. Danquah presided over the meetings as the official representative of Nana Ofori Atta (Akim Abuakwa State Council Session 1937).

The chiefs also chose to delegate considerable authority over judicial matters. Under the successive Native Administration Ordinances, the British created a number of native tribunals with authority over subjects loyal to that stool. The ordinances vested the tribunals with jurisdiction over both civil and criminal cases, but they did not outline the procedures for hearing these cases. The omission opened the door to widespread corruption. Chiefs throughout the colony began using the tribunals as money-making machines. They repeatedly adjourned cases so that they could collect a daily adjournment fee and routinely assessed the maximum penalties to all parties. Without an adequate treasury system, the fees and fines accrued directly to the tribunal's members (Gold Coast 1945).

For the Akyem chiefs, the profits earned from the corrupt behavior had to be weighed against other sources of revenue. They could abuse their power in the tribunals to earn income, but the rising number of complaints against the tribunals threatened their tenure in office. As the value of long-term tenure rose, the relative value of the tribunals decreased, and the chiefs themselves began looking for a means by which to make the tribunals operate more effectively.

Reforms proceeded along two fronts. First, the chiefs regularized procedures within the tribunals, limiting the opportunities for corrupt behavior. To this end, Ofori Atta became a leading advocate for the Native Administration Staff Association, which was inaugurated in 1939. The association pushed for the development of a professional staff of clerks and registrars to serve the tribunals. It also recommended that tribunals hire only literate men and that those hired should undergo regular training for their posts. The staff were to be paid salaries commensurate with these specialized skills so that they, too, would not be tempted to behave corruptly. Finally, the staff would be protected from "indiscriminate or wanton dismissal" by chiefs.

Second, Ofori Atta removed the chiefs from the tribunals, delegating their authority over judicial matters to citizens of the state. By 1942, the British had become alarmed by the corruption evident in the tribunals. The government appointed a commission of enquiry [the Blackall Commission] to outline strategies for change. Both Ofori Atta and Danquah sat on the committee.[6]

I was able to locate only fragmented accounts of the committee's deliberations, but even these are suggestive. Both Ofori Atta and Danquah sought to preserve a judicial system based on customary law rather than British law, but both wanted to open the tribunals to educated commoners, well versed in customary law. Lawyers would not be allowed to appear before the tribunals, because "lawyers care very little, if at all, for native customary law," and their presence would lead the courts to "lose their identity." If chiefs continued to sit on the tribunals, then they should receive only sitting fees, unrelated to the fees and fines imposed by the tribunal. The latter charges would be deposited directly into state treasuries (Minutes of the Blackall Committee Meetings 1943).

This evidence suggests that both Danquah and Ofori Atta saw the value of customary law. The right to interpret custom remained a powerful defense against the British and a means by which to control and communicate with the state's citizens. However, they also sought to transfer that right to what Danquah referred to as society's "new elements." Authority should be delegated, reassuring the state's citizens that those in the tribunal would no longer abuse their power.

Ultimately, many of the reforms Danquah and Ofori Atta promoted were written into the Blackall Report and included in the Native Courts Colony Ordinance (1944). Under the ordinance, the British created new native courts. Each court operated according to the customary law prevailing within its jurisdiction and employed a trained registrar to supervise other personnel. The registrar's appointment, dismissal, and terms of service were subject to the approval of the provincial commissioner. Court personnel were to be trained by a judicial advisor. Courts were allowed to open only in those states in which a native treasury operated, thus assuring that the court staff would receive payment, and that court officials could account for all fees and fines collected. Finally the court membership could be drawn from persons outside the traditional hierarchy (Native Courts Colony Ordinance 1944).

A large number of the traditional states in the Gold Coast Colony failed to take advantage of this final clause. In their 1950 review of the tribunal system, the Korsah Committee reported that 45% of court members in the colony were nonchiefs. The number in Akyem Abuakwa far exceeded the national figure. In 1946, the Grade A Court of Appeal panel consisted of seven commoners, one chief, and one okyeame (Native Courts Colony 1946). Further, the Okyenhene himself had appointed Danquah to serve as *otemfuo*, or superior court judge presiding over the court (Dua Sakyi 1992). Relative to the chiefs in other states, then, the Akyem chiefs had clearly demonstrated their willingness to delegate power and submit themselves to the authority of others.

THE EVOLUTION OF THE TREASURY: A TEST OF THE ARGUMENT

In the preceding pages, I have argued that Ofori Atta and Danquah initiated a series of reforms intended to enforce Ofori Atta's preferred property rights system. The reforms helped Ofori Atta to defeat those who advocated an alternative definition of property rights, gave the state's citizens greater control over the chief, and signaled the paramount chief's commitment to the new property rights regime. If the reforms were successful, then citizens would come to believe that their rights were secure under the existing land tenure system. Consequently, citizens would become more willing to invest on the basis of those rights.

To test the argument that the new institutions created a more secure property rights system, I examine the revenue and expenditure patterns from the Akyem Abuakwa State Treasury. Taxes paid to the treasury resemble investments in that there is a lag between the payment of taxes and profit in the form of public goods provision. Therefore I use tax payment as a proxy of an individual's willingness to invest.[7]

In spite of their early failure, Danquah and Ofori Atta continued to lobby for the creation of some form of native treasury system within Akyem Abuakwa, and in 1937 the Okyenhene Council passed a new bylaw authorizing the creation of the Akyem Abuakwa State Treasury. In 1938 their efforts were reinforced by the introduction to the legislative council of the Native Administration Treasuries Ordinance. Under the ordinance, a state council could, by resolution, establish both central and divisional treasuries within the state, and impose an annual rate or tax. Shortly after the treasury ordinance passed, the Okyeman Council opened a central treasury in Kyebi.

How did this new institution fare? My analysis yields the following hypotheses regarding the treasury's performance.

1. As divisional chiefs respond to the new incentive structure that they face, the revenue derived from the sale of land should decrease.
2. As the paramount chief and divisional chiefs demonstrate their commitment to the new property rights system, citizen tax payments to the treasury should increase as citizens exchange revenue for the enforcement of property rights.[8]
3. To cement the contract between the citizen and chief, (a) the chiefs should use the courts to provide a public good (the enforcement of property rights) rather than to generate income, and (b) the increase in revenues should be matched by an increase in expenditures for the provision of public goods.

I was able to locate nearly complete treasury returns for the period 1940–50.[9] Returns for 1939–40 were unavailable, though Lord Hailey records that receipts for that year totaled just £6,000. The figures tell a remarkable story. First, note that the revenue derived from land sales decreased sharply after the first year, falling from £2,500 to just £87 (Figure 1). They never recovered; for the remainder of the decade, revenue from land sales never exceeded £167.

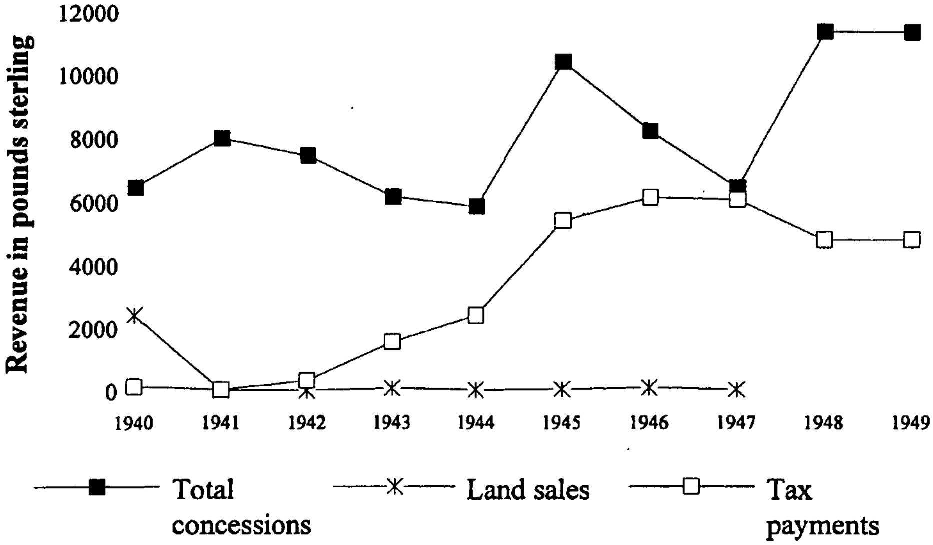
I argue that the decrease in the revenue from land sales can be linked to a shift in the chiefs' incentive structure. Although prices remained high throughout this period, the chiefs did not sell the land. The chiefs knew that they would be sanctioned for such action, and they preferred to receive the stable, long-term income offered by the maintenance allowance. An alternative hypothesis would attribute the decrease in revenue from sales to the chiefs' ability to hide their wealth, avoiding detection. Given the increase in other sources of revenue—notably tax payments and concessions payments, which had previously been a major source of income for the chiefs—that explanation seems unlikely. If the chiefs escaped detection or if the public failed to monitor the chiefs, then *all* revenue sources should have been affected.

As the revenue derived from land sales declined, tax payments to the treasury increased. During the first year of the treasury's operation, income from taxation totaled only £210, or 1% of total revenue collected (Figure 1); during the second year, that amount decreased to £112. By 1942–43, however, income from customary taxes totaled £396, an increase of 254%. The level of income from customary taxes increased dramatically in each of the following two years; it then leveled somewhat but continued to rise.

It is important to note that these increases do not simply mirror changes in the world price for cocoa. Treasury receipts began to climb in 1942, while world cocoa prices began to rise two years later, in 1944. Moreover, the treasury receipts remained relatively

FIGURE 1

State Treasury Revenue Sources, 1940–49



Source: Akyem Abuakwa State Archives, Files 5/81, 5/84, 5/85, 5/99, 5/111. Because these figures are compiled from four separate Akyem Abuakwa state files, the categories were not consistent across the time period. I have tried to duplicate the categories as faithfully as possible. The figure has been adjusted for inflation using 1940 as a base year.

stable from 1945 to 1949, even as cocoa prices soared (Bauer 1954). It would seem, then, that citizens and ruler had adopted a cooperative strategy: citizens paid their taxes to the treasury, and in return, rulers enforced property rights, refusing to appropriate the farmers' profits.[10]

Ultimately, individual citizens will continue to pay their taxes to the treasury voluntarily only if the strategy yields a positive payoff—that is, if the chiefs enforce the new property rights system and if the public tax payment is returned to them in the form of public goods. Again, the treasury returns suggest that the Akyem chiefs satisfied both conditions.

The expenditure figures indicate that the chiefs used public funds not to finance their private interests but to provide public goods to the state's citizens. In 1940–41, the state devoted fully £12,379 of its resources to state administration. A large portion of that served to renumerate chiefs and other stool functionnaires. In 1941–42, the total expenditures for the state's administration dropped by nearly half (Figure 2). In subsequent years, the absolute amount of revenue expended on state administration increased, reflecting the need to share the state's wealth with the chiefs to retain their cooperation; but the percentage of expenditure devoted to state administration decreased steadily, falling to £7,674 (23% of the total) by 1948–49 (Figure 2).

At the same time, the funds devoted to state development increased dramatically from a mere £1,774 in 1940–41 to a high of £18,656 in 1949–50 (Figure 2). The chiefs were, in fact, behaving as faithful agents of their subjects. Not only were they managing the public lands more efficiently, but they also used the profits from the land for the public good funding, inter alia, sanitation projects, public education, development of the state's infrastructure, management of timber reserves, and even construction of a post office. The terms of the original contract, embodied in the 1929 Stool Lands Declaration and Treasury By-Law, were finally complete.

The returns from the state treasury are even more remarkable, given the political and economic turmoil that prevailed in the 1940s. Ofori Atta I's death in 1943 sparked prolonged controversy in the state. A small number of Ofori Atta's supporters (including two sons) allegedly murdered the *odikro* of Apedwa and used the *odikro's* blood to honor the dead paramount chief by blackening his stool (Rathbone 1993, 70). During the ensuing investigation and trial, Ofori Atta's detractors sought to exploit the dispute to reclaim a more dominant position in the state.

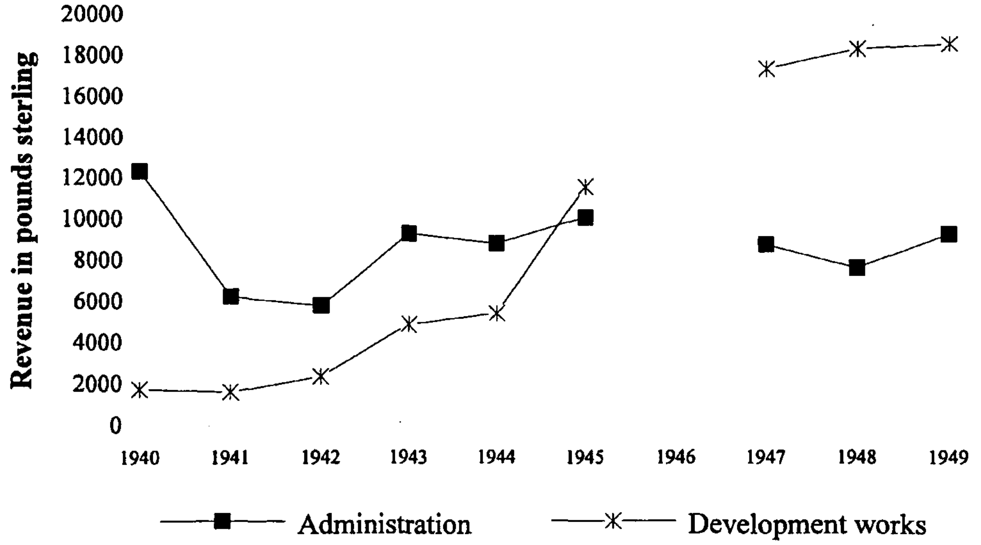
The political intrigue coincided with severe economic decline. In 1945, nearly a hundred square miles of Akyem Abuakwa's best cocoa-growing land was devastated by drought and the spread of swollen shoot disease (ibid., 175–76). In addition, the ongoing world war disrupted international trade, fueling both commodity shortages and high inflation throughout the colony.

Rathbone argues that with Ofori Atta's death, the key organs of the Akyem state began to atrophy. And indeed, in 1944, the British district commissioner expressed his disappointment that the state treasury had not lived up to the "great promise shown in previous years" (ibid., 176). Four years later, the colonial government threatened to withhold grant monies unless certain financial irregularities were corrected (pp. 177–78).

In spite of these difficulties, the treasury survived.

FIGURE 2

State Treasury Expenditure Items 1940–49



Source: Akyem Abuakwa State Archives, Files 5/81, 5/84, 5/85, 5/99, 5/111. Because these figures are compiled from four separate Akyem Abuakwa state files, the categories were not consistent across the time period. I have tried to duplicate the categories as faithfully as possible. The figure has been adjusted for inflation using 1940 as a base year.

A careful examination of the treasury's revenue and expenditure patterns suggests that following Ofori Atta's death, property rights in land remained secure. The institutional innovations put into place during Ofori Atta's reign outlived their founder and allowed Ofori Atta's successor to forge his own commitment to uphold property rights (Firmin-Sellers n.d., chaps. 5–6).

CONCLUSION

My analysis of the creation of property rights institutions in Akyem Abuakwa yields important insight into theories of economic development. Neoclassical economists contend that customary land tenure impedes economic growth, and champion a move toward private property rights. Other development scholars insist that customary land laws can promote productive investment and dismiss privatization as an expensive, unnecessary, and potentially disruptive process.

The study of the Akyem case demonstrates that the debate between these groups is ill-formulated, because it ignores the politics of enforcement. The productive potential of private property rights and customary land tenure depends upon the political institutions through which property rights are defined and enforced. Abstract debates over the efficiency of either system are irrelevant in the short term, because the desirability of either system depends upon the actors' ability to enforce property rights at the national or local level. Policy proposals that neglect the problem of enforcement are, therefore, incomplete.

Throughout contemporary Africa, state enforcement of private property rights has proven to be prohibitively costly and difficult to achieve. Given this fact, it behooves policymakers to investigate the effectiveness of local enforcement of customary land tenure. If customary land tenure is to promote sustained economic growth across the multitude of traditional areas, clans and lineages in Africa, then national governments, must take steps to strengthen local political institutions (Ostrom 1990). Government officials cannot simply record and disseminate information about current land use. Government officials must delegate real decision-making authority to local actors and enforce local-level decisions when disputes arise.

The study of the Akyem case also demonstrates that the ruler who attempts to enforce property rights must strike a delicate balance between coercive and cooperative institutions. In a local community or nation-state, property rights will not inspire individual investment and economic growth unless the ruler wields sufficient coercive authority to silence those who advocate an alternative, more distributionally favorable property rights system and force them to accept their losses. At the same time, the ruler must credibly commit him or herself to the new property rights system. The ruler must persuade the subjects that coercive authority will not be used to violate property rights and confiscate the subjects' wealth.

In Akyem Abuakwa, customary land tenure generated productive investment because Nana Ofori Atta manipulated both colonial and traditional institutions to satisfy these conditions. Within the colonial state, Ofori Atta positioned himself at the nexus of most contact between the paramount chiefs of the

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colony and British colonial officials. Ofori Atta used his control over the flow of information between these groups to persuade the British to use their coercive authority on his behalf, quelling domestic opposition to his preferred property rights system.

Ofori Atta also reformed the traditional state's political institutions. First, he created a state treasury system, making the state's citizens the residual claimants to the profits from stool lands. Second, he opened joint decision-making institutions and delegated judicial and financial authority to independent political bodies, establishing a credible commitment to uphold the new property rights system.

My analysis of property rights institutions in Akyem Abuakwa also yields insight into theories of institutional creation. New institutionalist scholars often depict institutional creation as a contractarian process (see Ostrom 1990; Taylor 1987). Actors in a community create institutions in order to minimize transaction costs, resolve collective action problems, and realize mutually beneficial gains. Critics of this approach contend that institutional creation is inherently coercive and conflictual. One or more actors in a community create institutions in order to extract private benefits from society. These institutions lead to productive outcomes only if the private interests of the powerful few coincide with the public interest. More often, however, such institutions lead to economic decay (Knight 1992; Moe 1990a, 1990b; North 1990).

This research demonstrates that neither of these extreme positions is tenable. Institutional creation is both coercive and contractarian in nature. It is coercive because actors rely on a coercive authority to halt the most egregious conflict over the institution's design. It is cooperative because, when the new institution is in place and disadvantaged actors accept their losses, people contract with one another to achieve mutually beneficial gains from specialization and trade. To be sure, individuals will continue to battle one another to secure a favorable share of those gains, but their competition is less likely to challenge or undermine the existing institutional arrangement.

If either coercion or cooperation is absent, then the institution will be weak. If no actor wields sufficient coercive authority to end distributional conflict, then it is extremely difficult for individuals in a community to uphold an institution. At the same time, if those with coercive authority refuse to restrict their use of force, then citizens will never adhere to the institution's ruler. Instead, they will search for ways to circumvent or undermine the institution, rendering it fragile and ineffective.

Notes

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1. I could not locate data detailing the actual price of these land sales, but evidence from the mining concessions negotiated with European firms is suggestive. Ofori Atta complained bitterly that mining companies had acquired their concessions for outrageously low rents—often as little as several bottles of schnapps or whiskey. The chiefs' willingness to grant these concessions could be interpreted as evidence of their disinterest in material profit (unlikely, given their rush to sell farm land) or as evidence of their addiction to European alcohol. It is equally likely, however, that the prices reflect the severe depression in prices caused by competition on the supply side. The 1961 *Report of the Commission of Enquiry into Concessions* contains a partial listing of the concessions granted in the Gold Coast since 1926.

2. Addo-Fening (1980) attributes the decline in the paramount stool's authority to the influence of European missionaries and to the effects of British indirect rule.

3. Danquah's participation in the reform process is best understood within the context of indirect rule. Colonial officials believed the Western-educated elite to be "detribalized troublemakers" who could not represent the uneducated rural masses. Consequently, colonial officials relegated the educated elite to a relatively minor role in colonial institutions and systematically discounted the elite's public pronouncements. Elsewhere, I have argued that these conditions drove the educated elite to pursue their political and economic goals through indigenous institutions. Danquah and others like him invested heavily in traditional institutions, maintaining ties to traditional leaders and even holding traditional offices themselves (Firmin-Sellers n.d.).

4. Ofori Atta's reforms systematically transferred authority away from the Amantoomiensa and to the Okyenhene's state council. Indeed, the 1927 Native Administration Ordinance did not even recognize the Amantoomiensa as a traditional body.

5. Only the ruler who believes that he or she either will be in office for a lengthy period or can appoint a successor has an incentive to enforce property rights. In either case, the ruler places a high value on the future returns from office holding. See Levi 1988; North and Weingast 1989; and Olson 1993.

6. Other members of the committee included H. W. B. Blackall (chair), T. R. O. Mangin, Justice Woolhouse Bannerman, Nene Mate Kole (the *konor* of Manya Krobo), and J. C. deGraft Johnson.

7. It is important to note that this examination of the revenue and expenditure patterns does not compare the level of investment under customary land tenure to the level of investment one might expect under a system of private property rights. Instead, the data measure the level of investment in the absence and presence of institutions to enforce property rights. The data therefore demonstrate the importance of enforcement to the study of land tenure.

8. An alternative hypothesis links the increase in revenue to a coincident increase in the government's use of force to collect taxes. Coercion, however, is never fully effective. Moreover, it is an extremely costly means of achieving a significant revenue increase (Levi 1988).

9. I have adjusted the treasury returns for inflation, which was rampant during and after World War II. Absent a more sophisticated inflation index, I used P. T. Bauer's (1954) measure of the price for all cotton piece-goods. The measures are roughly comparable to the partial figures cited in Austin 1964.

10. Note that beginning in 1948, the state switched from a system of taxation based on land (abusa, stool farm, ground rents) to an annual rate imposed on the individual citizen. The latter system may promise more revenue, but if opposed, it is enormously difficult to enforce. Land is an immovable, highly

visible product. The individual, however, can more easily exit a regime seeking to tax or simply hide him- or herself when a rate collector tries to assess the tax. Ceteris paribus, taxing an individual entails much higher monitoring and enforcement costs. That the state administration judged such a switch possible suggests that they believed cooperation was now a robust outcome.

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ECONOMIC CRISIS AND POLITICAL REGIME CHANGE: AN EVENT HISTORY ANALYSIS

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I examine the effect of economic crises on domestic political regime change. Using a statistical technique known as event history analysis and a new data set that identifies all instances of regime change in the 97 largest Third World countries, I develop multivariate models of democratic breakdown and democratic transition. My main findings are that inflationary crises inhibited democratization from the 1950s through the early 1970s but may have facilitated it in the late 1980s and that recessionary crises facilitated democratic breakdown but had no effect on democratic transition throughout this period. The inflation findings—though not the recession findings—support the arguments of Karen Remmer and Samuel Huntington that the processes affecting democratization were very different in the 1980s than in earlier eras. A number of other explanatory variables emerge as significant determinants of regime change, providing support for several other contentions that have appeared in the literature.

One of the most prominent subjects in comparative politics during the past few decades has been the relationship between economic crisis and domestic political regime change. Beginning with the pathbreaking work of Guillermo O'Donnell (1973), many authors have developed complex theoretical models focusing on the role of economic crises in triggering breakdowns of democracy. Some of these authors argue that economic crises trigger democratic breakdown in conjunction with certain background conditions. Others argue that economic crises can also trigger regime change in the opposite direction—transitions *to* democracy. Some have even suggested that while economic crises triggered democratic breakdown in the past, they no longer do so. Much of the most recent work on regime change—including O'Donnell's—takes a very different analytical approach. Nevertheless, the idea that economic crises can trigger regime change remains widely accepted.

Despite the wide acceptance of this idea, empirical research on this subject has been limited mainly to case studies focusing on a few advanced Latin American countries. These case studies have proven inconclusive, and little effort has been made to examine this subject in a broader sample of countries. As a result, many key questions remain unanswered: Is there a systematic relationship between economic crisis and regime change? Do economic crises trigger both democratic breakdown and democratic transition? Do they trigger breakdown or transition in conjunction with certain background conditions? Have their effects changed over time? Are the determinants of breakdown and transition fundamentally similar or different?

I address these questions here by analyzing a large cross-national data set with statistical methods. Because regime change is an inherently dynamic phenomenon, the analysis uses time series data for 75 Third World countries and a regressionlike statistical technique known as *event history analysis* that enables me to analyze this data set rigorously. Several important findings emerge, providing interesting insights into these questions and revealing a number of fruitful avenues for further research.

ECONOMIC CRISIS AND REGIME CHANGE

Prior to O'Donnell's (1973) initial work on this subject, most of the literature on the determinants of political regime type and regime change focused on broad structural factors that were thought to be conducive to either authoritarianism or democracy. The most widely studied factors of this sort were a series of *socioeconomic conditions* linked to economic development or "modernization," including the level of per capita income, the extent of literacy and education, the degree of urbanization, and the quality and extent of communications media. According to "modernization theory," low levels of these factors are conductive to authoritarianism and higher levels are conducive to democracy. Because these factors are closely related to the level of economic development, countries undergo a gradual, inexorable transition from authoritarianism to democracy as their economies develop, according to these authors (Deutsch 1961; Lerner 1958; Lipset 1959; see also Burkhart and Lewis-Beck 1994). In a related vein, many authors have argued that capitalist economic development creates growing social pressure for democratization by fostering the emergence of a middle class (Lipset 1959), bourgeoisie (Moore 1966), or working class (Rueschemeyer, Stephens, and Stephens 1992) that seeks access to state power.

In addition to these development-related socioeconomic factors, some authors emphasize the importance of *social-structural conditions* such as societal homogeniety, low or moderate inequality, a relatively even distribution of power among societal groups, and overarching loyalties or cross-cutting cleavages as factors that facilitate democracy (Dahl 1971; Lijphart 1977; Muller 1988; Vanhanen 1990). Others focus on *political culture*, arguing that traits such as tolerance, trust, egalitarianism, and a willingness to compromise are preconditions for democracy (Almond 1980; Diamond 1993). These traits are said to make Protestantism more conducive to democracy than Catholicism, Islam, Confucianism, or Buddhism (Bollen 1979; Huntington 1984).

Another set of structural factors that can affect political regimes is the *character of political institutions*. Under democratic regimes, institutional features that promote stability and compromise are widely thought to facilitate the persistence of democracy, including the extent of institutionalization (Huntington 1968), consociational arrangements (Lijphart 1977), coherent (nonfragmented) party systems (Mainwaring 1993), and parliamentary rather than presidential systems (Linz 1994). Huntington's (1968) argument about the importance of institutionalization also applies under authoritarian regimes, but consociationalism, party system structure, electoral rules, and the type of executive system are largely irrelevant and therefore presumably have little effect on democratization under these regimes. The most distinctive and most variable institutional feature of authoritarian regimes is the role played by the military in politics. Much of the early literature on military intervention in politics emphasized the exceptional, temporary nature of this intervention (see Rouquié 1986, 108–9), implying that military-led authoritarian regimes are more likely to relinquish power and perhaps also more likely to permit democratization than those led by civilians. More recent literature has challenged this view, arguing that a "new professionalism" (Stepan 1973) or ideosyncratic conditions (Rouquié 1986) have often led the military to play a much more permanent role in politics and therefore presumably to be *more* resistant than civilians to relinquishing power and permitting democratization to occur.

Finally, some authors have identified *international political and economic conditions* that may affect political regimes, including colonial legacies (Collier 1982; Weiner 1987, 19–21), economic dependence (Bollen 1983; Gasiorowski 1988; Gonick and Rosh 1988), relationships with superpowers (Gasiorowski 1991; Muller 1985), the "demonstration effect" of democracy in neighboring countries (Huntington 1991, 100–106), and other aspects of the international environment (Gourevitch 1978; Jackson and Rosberg 1982; Whitehead 1986).

A large body of empirical research has shown that many of these structural factors significantly affect political regimes (Arat 1991; Cnudde and Neubauer 1969; Hadenius 1992; Vanhanen 1990). However, theories of regime change that focus only on structural factors of this sort are inherently problematic: although they identify factors that may *facilitate* regime change, they do not consider the *processes* that actually bring it about and therefore cannot fully explain its *causes* (Rustow 1970). These processes, typically involving coups d'état that result in military rule or mass protests leading to democracy, consist of "the strategic behavior of political actors embedded in concrete historical situations" (Przeworski 1986, 47). Although much of the recent work on regime change focuses narrowly on this "strategic behavior" (Di Palma 1990; Kitschelt 1992; O'Donnell, Schmitter, and Whitehead 1986), a much larger body of literature examines "concrete historical situations" that affect regime change. This literature emphasizes two types of concrete historical situation: structural factors that affect a society's propensity for regime change and momentous contemporaneous events such as economic crises or war that *trigger* processes of regime change. Some of these studies argue that regime change is especially likely to occur when *both* kinds of concrete historical situation exist—when triggering events occur *in conjunction with* certain structural factors that act as background conditions which *magnify* the effect of these events on regime change.

The most widely cited study of this kind is O'Donnell's (1973) monograph, which argued that the breakdown of democratic or semidemocratic regimes in Brazil and Argentina in the mid-1960s was triggered by economic crises caused by the exhaustion of the "easy" stage of import substitution industrialization (ISI) and the inability of these countries to "deepen" their economies and thus undertake the "hard" stage of ISI under "incorporating" regimes. According to O'Donnell, certain powerful political actors believed these crises could only be resolved by replacing these regimes with "exclusionary" bureaucratic-authoritarian regimes, because incorporating regimes were unable to carry out the painful measures needed to implement the hard stage of ISI. O'Donnell implicitly argued that a necessary background condition for the breakdown of these incorporating regimes was a relatively high level of economic development, which fostered both the exhaustion of the "easy" stage of ISI and the emergence of a politicized working class that demanded high wages. Within this context, economic crises were incompatible with incorporating regimes and therefore triggered the breakdown of these regimes. O'Donnell (1978, 1988) later modified this argument and applied it to other countries.

Another seminal study of this sort is Linz's (1978) monograph, which argued that breakdowns of democracy occur when incumbent governments are unable to solve certain critical problems, producing legitimacy crises that lead actors who are "disloyal" or "semiloyal" to democracy to destroy the democratic regime. Linz identifies a number of background conditions that may facilitate this process, including societal heterogeneity, a presidential system, a fragmented party system, the prevalence of ideological rather than pragmatic leaders, and the exclusion of leaders associated with previous authoritarian regimes. The "unsolvable problems" that trigger these democratic breakdowns include, but are not limited to, economic crises.

These pathbreaking studies led a generation of scholars to study regime change in a similar manner (Bermeo 1990). Although O'Donnell's work has been widely criticized (Collier 1979; Duty 1991; Remmer and Merkx 1982), many scholars have emulated it by examining how tensions rooted in economic development processes may trigger democratic breakdown (Crowther 1986; Im 1987; Kaufman 1979; Kurth 1979). Other scholars, seemingly inspired by O'Donnell, portray democratic breakdown as a consequence of economic crises caused by very different structural economic conditions, such as transitions between stages of capitalism (Poulantzas 1974), economic dependence (Szymanski 1981, 449–50; Thomas 1984), and the Third World debt crisis (Roett 1985). Departing more clearly from O'Donnell's structuralist approach, scholars such as Kaufman (1976), Skidmore (1977), and Wallerstein (1980) have argued that economic crises involving slow or negative growth or high inflation—whatever their causes—may trigger breakdown. Most of these authors imply that breakdown occurs most often in countries in the "dangerous middle" stages of development, where political instability is rife because the lower and middle classes are politicized but still have low living standards (Chirot 1977, 76–77).

Following the establishment of democratic regimes in many Third World countries in the early and mid-1980s, a number of authors developed similar arguments in which economic crises were said to trigger transitions *to* democracy (Epstein 1984; Richards 1986; Markoff and Baretta 1990). These authors argue that economic crises can undermine the legitimacy not only of democratic regimes but also of authoritarian regimes, thus triggering not only democratic breakdown but more generally *regime change*, regardless of what type of regime currently exists. Many authors who follow a process-oriented approach also cite economic crises as possible catalysts of democratic transition, even if they do not explicitly incorporate these crises into their models (Baloyra 1987; Malloy and Seligson 1987; O'Donnell, Schmitter, and Whitehead 1986).

Although these arguments that economic crises trigger democratic transition superficially resemble the models in which economic crises are said to trigger democratic breakdown, they contradict these models at a deeper level. O'Donnell (1973) and others *do not* argue that economic crises undermine the legitimacy of democratic regimes and thus trigger breakdown. Rather, they argue that certain political actors who are capable of bringing about regime change believe that democratic and semidemocratic regimes hinder the resolution of these crises because governments under such regimes are more responsive to popular pressure and therefore less capable of carrying out the painful measures needed to resolve these crises; these actors thus conclude that democratic breakdown is necessary. By implication, economic crises occuring under nondemocratic or semidemocratic regimes should lead the corresponding actors similarly to oppose democratization efforts, *inhibiting* rather than facilitating democratic transition under these circumstances. Thus while the first line of reasoning holds that economic crises undermine the legitimacy of whatever type of regime currently exists and therefore trigger regime change in either direction, the second implies that economic crises are incompatible with democracy and therefore have different, though *complementary*, effects on regime change in each direction, facilitating democratic breakdown but inhibiting democratic transition. The structural factors that are thought to be conducive to authoritarianism or democracy also presumably have complementary effects on the likelihood of regime change in each direction, facilitating democratic breakdown and inhibiting democratic transition, or vice versa.

Another variation on the relationship between economic crisis and regime change is offered by Remmer (1990), who argues that the foreign debt crises that occurred in Latin America in the 1980s did not trigger democratic breakdown, as earlier economic crises had, because certain background conditions were very different in this period. These different conditions include greater efforts by the United States to promote democracy, other changes in the international political context, new attitudes toward democracy among business and military elites, more pragmatic and inclusive approaches to democratic governance, and the legacy of prior events in certain countries. These different conditions offset the adverse impact of economic crises on democracy and even facilitated democratic transitions, according to Remmer. Huntington expresses similar ideas at a more general level, arguing that a "third wave" of democratization that was very different from the previous waves began in the mid-1970s as a result of the spread of democratic norms, more active efforts by the Western powers and the Catholic Church to promote democracy, economic crises and other performance problems in nondemocratic regimes, changes in socioeconomic conditions, and other factors (1991, 40–108).

This discussion suggests six hypotheses that can be examined empirically:

HYPOTHESIS 1. *Economic crises trigger democratic breakdown.*

HYPOTHESIS 2. *Economic crises trigger democratic transition in a manner that is fundamentally similar to their effect on democratic breakdown.*

HYPOTHESIS 3. *Economic crises facilitate democratic breakdown but inhibit democratic transition in a complementary manner, contradicting hypothesis 2.*

HYPOTHESIS 4. *The role of economic crises in triggering democratic breakdown declined over time and became insignificant in the 1980s, while their role in triggering democratic transition increased and became significant during this decade.*

HYPOTHESIS 5. *Socioeconomic and social-structural conditions, political culture and institutions, and international conditions affect democratic breakdown and democratic transition, in each case facilitating regime change in one direction and inhibiting it in the other, in a complementary manner.*

HYPOTHESIS 6. *Economic crises trigger democratic breakdown, and possibly also democratic transition, in conjunction with a relatively high level of development, the social and political conditions discussed by Linz, and perhaps other structural factors as well.*

The remainder of this study seeks to evaluate these hypotheses.

EMPIRICAL MEASURES AND SAMPLE

To study the role of economic crises as triggers of democratic breakdown and transition, we first need to identify the distinct points in times at which political regimes changed from democratic to nondemocratic (or vice versa) in an appropriate sample of countries. Most existing quantitative measures of democracy are based on continuous, ordinal scales that rate countries according to their "degree of democracy." Although measures of this sort have certain advantages (Bollen 1990), their main drawback for our purposes is that they do not clearly distinguish democratic from nondemocratic regimes and therefore do not permit us unambiguously to identify distinct instances of regime change. For example, Bollen's (1980) measure of democracy rates Brazil at 90.5 in 1960 and 60.9 in 1965 on a hundred-point scale—values that do not clearly identify Brazil's 1964 coup as an instance of regime change. With such a measure, regime change can only be identified by assuming that movements along the scale of a certain magnitude (e.g., 20 points) or past a certain value (e.g., 80) denote instances of regime change. Assumptions of this sort are arbitrary and therefore not satisfactory.[1]

In order to carry out this study, I developed a data set featuring a categorical measure that distinguishes democratic and nondemocratic regimes fairly clearly and thus permits us to identify distinct instances of regime change.[2] This measure classifies political regimes as either democratic, semidemocratic, or nondemocratic.[3] I chose to use three categories rather than two because it was evident that many countries have regimes that are neither fully democratic nor truly nondemocratic and that distinct changes to and from these "semidemocratic" regimes do indeed occur. I collected data on this measure for the 97 Third World countries with populations of at least one million in 1980 for the periods beginning with their independence or the date at which a "modern" state[4] first appeared and continuing through 1992. I focused only on Third World countries because almost all regime changes in the post–World War II era (when systematic quantitative data are more widely available) have occurred in these countries. I excluded small countries because they are often highly anomalous and therefore might distort the analysis.

My definitions of the three regime categories are based largely on Diamond, Linz, and Lipset (1990, 4:xvi–xvii):

Democracy. A regime in which (1) meaningful and extensive competition exists among individuals and organized groups for all effective positions of government power at regular intervals and excluding the use of force, (2) a highly inclusive level of political participation exists in the selection of leaders and policies such that no major (adult) social group is excluded, and (3) a sufficient level of civil and political liberties exists to ensure the integrity of political competition and participation.

Semidemocracy. A regime in which (1) a substantial degree of political competition and freedom exist but where the effective power of elected officials is so limited, or political party competition is so restricted, or the freedom and fairness of elections are so compromised that electoral outcomes, while competitive, still deviate significantly from popular preferences and/or (2) civil and political liberties are so limited that some political orientations and interests are unable to organize and express themselves.

Nondemocracy. A regime in which little or no meaningful political competition or freedom exists.

Using these definitions, I carefully examined a variety of historical sources[5] to determine when the political regime in each country changed from one of these three categories to another. This process yielded annual time series of varying lengths for the 97 countries, indicating what type of regime existed in each year and when regime changes occurred. In most cases distinct instances of regime change were easy to identify because they occurred through a change of government (typically following a coup d'état or free elections) or through the declared initiation or conclusion of a state of emergency or some nondemocratic policy. In a few cases regime change occurred gradually rather than through a distinct event—typically, as a government became increasingly repressive toward its opponents. While it was therefore difficult to pinpoint the *exact dates* of regime change in these cases, it was never hard to make a judgement about the *years* in which they occurred. Since years are my temporal unit of analysis, I am confident that my judgments were adequate for the purposes of this study. (A complete listing of these data and a more detailed description of the data collection procedure are provided in Gasiorowski n.d. Narrative profiles for each country giving the main historical details that guided my classifications are available upon request.)

The explanatory variables used in the analysis, together with their mnemonic variable names, are as follows:

Economic Crisis Variables: inflation rate (INFLATION), real economic growth rate (REGROWTH)
Socioeconomic and Social Structure Variables: real income per capita (INCOMEPC), ethnolinguistic fractionalization index (ELFI)
Political Culture Variables: Catholic percentage of population (CATHPOP), Moslem percentage of population (MOSLEMPOP)
Political Institution Variables: presidential system (PRESIDENTIAL), military regime (MILREGIME), institutionalization/duration of current regime (REGIMEDURATION), institutionalization/prior years under democratic or semidemocratic regime (DSYEARS)
International Variables: trade dependence (TRADEDEPENDENCE), proportion of countries in the region that are currently democratic (REGIONDEM).
Miscellaneous: calendar year (YEAR)

The two economic crisis variables (INFLATION and REGROWTH) measure the severity of inflationary and recessionary crises, respectively—the main types of economic crisis discussed in the literature reviewed. I constructed these measures by calculating the two-year moving averages of the inflation and real economic growth rates. Thus their values in year y are the averages of these underlying variables in years y and $y - 1$.[6] I used these two years mainly on the assumption that political actors' behavior is based on their assessments of economic conditions prevailing not only at the current time but also in the recent past and near future. While regime changes obviously occur throughout the current year, these measures, on balance, give the average inflation rate and growth rate in the 18 months preceding a regime change and the 6 months following it—intervals that seem appropriate. Moreover, since the underlying inflation and economic growth data were not available in some cases prior to year $y - 1$, moving averages of longer duration or covering earlier periods would have reduced the sample size. Since these moving averages mainly cover the periods preceding regime change, they have the additional advantage of ensuring that we are studying the causal effects of inflation and economic growth on regime change, not the reverse.

In addition to these two economic crisis variables, I included in the analysis explanatory variables corresponding to all of the discussed structural factors that could be easily measured and would yield a fairly large number of observations. I did this for three reasons: (1) to enhance specification of the multivariate models by providing control variables in the analysis; (2) to provide additional insight into the causal processes affecting regime change; and (3) because the conjunctural models of regime change implicit in the work of O'Donnell, Linz, and others raise the possibility that *any* of the structural factors discussed above might magnify the impact of economic crises on regime change in a similar manner.

Real income per capita (INCOMEPC) is a proxy for the development-related socioeconomic factors. The ethnolinguistic fractionalization index (ELFI) is a measure of societal heterogeneity. The variables giving the percentage of a country's population that is Catholic (CATHPOP) and Moslem (MOSLEMPOP) are proxies for political cultural factors associated with these religions. PRESIDENTIAL and MILREGIME are dummy variables indicating whether the country has a presidential system and whether the executive branch is controlled by the military. REGIMEDURATION gives the number of years since the current regime was established (a proxy for the age of the regime's political institutions, serving as a rough measure of institutionalization). DSYEARS gives the number of prior years in which a democratic or semidemocratic regime existed in the country and serves as a rough measure of the strength of democratic institutions. TRADEDEPENDENCE is the sum of a country's imports and exports divided by its gross domestic product (the size of a country's foreign-trade sector relative to the size of its economy, widely used to measure the extent to which the economy depends on trade). The proportion of countries in the region that are democratic (REGIONDEM) is a proxy for the "demonstration effect" of democratic neighbors.[7] I include calendar year (YEAR) to examine whether the conditions affecting democratization changed during the period under study. Because INFLATION, REGROWTH, INCOMEPC, REGIMEDURATION, and DSYEARS are very skewed at the upper ends of their ranges, I use their natural logs in the analysis.[8]

This set of explanatory variables, though fairly large, excludes many structural factors that are thought to affect democracy. Some of these factors, such as cross-cutting cleavages and certain aspects of the international environment, are not readily quantifiable; others, such as inequality and party system fragmentation, have been quantified but are not available for many countries. Moreover, some of the variables used here, including notably the two variables dealing with institutionalization, are rather crude measures of very complex phenomena, while others are undoubtedly subject to measurement error. These data limitations make it difficult or impossible to study the effect of certain structural factors on regime change and raise the question of specification error in the multivariate models. However, the variables that are included in the analysis cover most of the factors that are generally thought to have the greatest effect on regime change, so these limitations are probably not too severe.

The economic crisis variables and some of the other explanatory variables had many missing observations. Therefore, of the 4,351 observations from 97 countries in the post-1945 period for which regime change data were available, only 1953 observations from 75 countries had valid data for all of the explanatory variables.[9] Table 1 contains a transition matrix showing the number of regime changes that occurred in these 1953 observations *from* the type of regime listed on the left *to* the type listed at the top. The entries in the off-diagonal cells of Table 1 indicate that 80 regime changes of the six possible types actually occurred. The countries and years in which these 80 regime changes occurred are listed in the Appendix.

TABLE 1

Transition Matrix of Regime Changes in 75 Third-World Countries, 1950s through 1980s

	NUMBER OF TRANSITIONS TO			
FROM	DEMOCRATIC REGIME	SEMIDEMOCRATIC REGIME	NONDEMOCRATIC REGIME	TOTAL
Democratic regime	(385)	3	20	408
Semidemocratic regime	3	(146)	16	165
Nondemocratic regime	21	17	(1342)	1380
Totals	409	166	1378	1953

Since the number of regime changes of each type is really too small for statistical analysis, I decided to combine the 39 regime changes shown in the upper-right corner of Table 1 that occurred in a less-democratic direction and the 41 in the lower-left corner that occurred in a more-democratic direction, yielding two data sets. The first data set contains a dichotomous dependent variable whose value is 1 for the 39 observations involving less-democratic regime change (*democratic breakdowns*) and 0 for the 534 democratic and semidemocratic observations in which breakdown could have occurred but did not. The second data set contains a similar dichotomous dependent variable whose value is 1 for observations containing the 41 *democratic transitions* that produced more-democratic regimes and 0 for the 1,504 semidemocratic and nondemocratic observations in which democratic transitions could have occurred but did not.

Both data sets consist of multiple time series of varying lengths from the cross-sectional sample of 75 countries. Some of these time series culminate in democratic breakdowns or transitions; others are "censored" when data become unavailable before the next breakdown or transition occurs. Both data sets are fairly well distributed over the major regions of the Third World and over time: 10 breakdowns in Latin America, 17 in Subsaharan Africa, 4 in the Middle East and North Africa, 5 in South Asia, and 3 in East Asia; 4 in the 1950s, 16 in the 1960s, 13 in the 1970s, and 6 in the 1980s. The corresponding numbers of democratic transitions are 18 transitions in Latin America, 9 in Subsaharan Africa, 5 in the Middle East and North Africa, 3 in South Asia, and 6 in East Asia; 4 in the 1950s, 8 in the 1960s, 12 in the 1970s, and 17 in the 1980s.

STATISTICAL METHODOLOGY

The time-series cross-section data set and dichotomous dependent variables employed here rule out the use of standard multivariate regression techniques. Fortunately, a family of statistical techniques known as event history analysis has been developed that enables researchers to carry out multivariate analysis in a research design of this sort (Allison 1984; Blossfeld, Hamerle, and Mayer 1989; Tuma and Hannon 1984). Event history analysis is used to study the occurrence of distinct events (e.g., marriages, changes in occupation, political change of some kind) within observed historical periods for a group of subjects. The occurrence and nonoccurrence of the event is treated as a dichotomous dependent variable, which is observed over contiguous time periods of varying lengths for each subject in the cross-sectional group. Explanatory variables thought to affect the likelihood, or *odds*,[10] of the event occurring are also observed for each subject in these time periods and serve as independent variables in regressionlike multivariate models. When observations are measured in discrete units such as years, multivariate logit (or logistical regression) analysis can be used to estimate the coefficients, standard errors, and covariances of such a model (Allison 1984, 14–22). In logit analysis, the hypothetical dependent variable is the natural logarithm of the odds—the log(odds)—that the event actually occurs. Since odds cannot be observed, we use in their place the dichotomous variable indicating whether or not the event occurred.

In this study, the event in question is either democratic breakdown or democratic transition and the explanatory variables are those listed earlier. As in regression analysis, t-tests indicate whether each explanatory variable significantly affects the dependent variable. To examine whether economic crises trigger regime change in conjunction with certain background conditions, we can multiply the appropriate pairs of explanatory variables and include the product terms (generally known as interaction terms) and the two component variables in the logit model. If an interaction term coefficient is significant, the corresponding explanatory variables affect the dependent variable *conjuncturally* in the sense that the presence of one magnifies the effect of the other. If the interaction term is not significant but the coefficients of the two component variables are, these variables affect the dependent variable independently of one another. To reduce multicollinearity, the two component variables are usually centered by subtracting out their means before the interaction term is calculated. This does not affect the coefficient estimates, standard errors, or covariance estimates of the logit model (Aiken and West 1991, 9–47).

To evaluate hypothesis 2, we can test whether the coefficients of the economic crisis variables in a logit model of democratic breakdown are significantly different from the corresponding coefficients in a logit model of democratic transition; if they are not, we can conclude that economic crises have similar effects on the two types of regime change. When multivariate models with identical sets of explanatory variables are estimated on different samples, we can test whether their coefficients differ by pooling the two samples together and estimating on the pooled sample a model that contains (1) an intercept term, (2) each of the explanatory variables, (3) a dummy variable whose value is 0 for observations in the first sample and 1 for those in the second, and (4) each of the explanatory variables multiplied by this dummy variable. The coefficients of this last group of variables will have values equal to the differences between the corresponding coefficients in the two separate models. The t-statistics of these coefficients therefore test whether these differences are significantly different from zero, that is, whether the two coefficients differ significantly from each other.

We can use a similar technique to evaluate hypotheses 3 and 5. If an economic crisis variable or structural variable facilitates democratic breakdown and inhibits democratic transition, it should have a significantly positive coefficient in a logit model of transition. We can test whether this is so simply by examining the signs and significance levels of the appropriate coefficients in the two logit models. To examine whether a given variable has a facilitating effect on regime change in one direction that is more, less, or comparable in magnitude to its inhibiting effect on regime change in the other direction, we can compare the size of one coefficient with the inverse of the size of the other. We can do this by following the procedure discussed in the previous paragraph but (1) replacing the intercept term with a variable equal to −1 for observations in the first sample and 1 for those in the second and (2) reversing the signs of the explanatory variables for observations in the first sample. The coefficients of the group 4 variables will then have values equal to the sums of the corresponding coefficients in the two models. The t-statistics of these coefficients test whether these sums differ significantly from zero, that is, whether one coefficient differs in size from the inverse of the other.

EMPIRICAL ANALYSIS

Table 2 presents event history logit models of the likelihood of democratic breakdown and democratic transition. Each column gives coefficient estimates corresponding to the explanatory variables listed on the left, together with their standard errors (beneath them in parentheses) and superscripts indicating their level of statistical significance. Positive signs on the coefficients imply that the corresponding variables *increase* the likelihood of breakdown or transition; negative signs imply that they *reduce* it. N_1 and N_0 give the number of 1 and 0 values of the dependent variable in each model. The −2 log likelihood and percentages of concordant and discordant predictions are measures of the explanatory power of each model.[11]

We can infer from the literature reviewed above that all the explanatory variables except MILREGIME should affect the viability of democratic and semidemocratic regimes. Moreover, except for the possibility that the economic crisis variables act in conjunction with some of the other variables, there is no reason to assume that a hierarchical causal structure of some sort exists among these variables. I therefore began my analysis of democratic breakdown by estimating a logit model that includes all of these variables except MILREGIME. As shown in model 1 of Table 2, the coefficient of log(INFLATION) in this model is not significant at even the .10 level, implying that high inflation does not independently affect the likelihood of breakdown. However, the coefficient of log(REGROWTH) is negative and significant at better than .01, indicating that slow or negative economic growth increases the likelihood of breakdown.

To examine whether high inflation and slow or negative growth affect breakdown in conjunction with any of the nine structural variables or YEAR, I added the 20 corresponding interaction terms separately to model 1. The only one that was significant was log(INFLATION)*YEAR. As shown in model 2, this interaction term had a significantly negative coefficient, implying that high inflation increased the likelihood of breakdown in the early part of the period covered by the sample (1950–89) but not in the latter part of this period. This indicates that the insignificant coefficient of log(INFLATION) in model 1 misrepresented the effect of inflation on breakdown because it did not pick up the time-varying character of this effect. Log(REGROWTH) remains significant in model 2, indicating that slow or negative growth increases the likelihood of breakdown even after the time-varying effect of inflation is taken into account. The significantly negative coefficient of REGIONDEM in model 2 indicates that democratic breakdown is less likely to occur when many countries in the surrounding region are democratic, presumably as a result of the "demonstration effect." The coefficient of log(INCOMEPC) in model 2 is negative but significant only at the .11 level, indicating that high income per capita has only a very marginally negative effect, at best, on the likelihood of breakdown.

We can get a clearer understanding of how the effect of inflation on democratic breakdown varied over time by rewriting model 2 as follows:

$$\begin{aligned}\log(\text{odds}) &= -3.295 + .917*\log(\text{INFLATION}) \\ &\quad - .066*\text{YEAR} - .092*\log(\text{INFLATION}) \\ &\quad *\text{YEAR} + \text{everything else} \qquad (1)\\ &= -3.295 + (.917 - .092*\text{YEAR}) \\ &\quad *\log(\text{INFLATION}) - .066*\text{YEAR} \\ &\quad + \text{everything else.} \qquad (2)\end{aligned}$$

TABLE 2

Logit Models of Democratic Breakdown and Democratic Transition

EXPLANATORY VARIABLE	DEMOCRATIC BREAKDOWN MODEL 1	DEMOCRATIC BREAKDOWN MODEL 2	DEMOCRATIC TRANSITION MODEL 3	DEMOCRATIC TRANSITION MODEL 4
Intercept	−3.531***	−3.295***	−2.951***	−4.443***
	(.307)	(.317)	(.320)	(.276)
Log(INFLATION)	.392	.917#	−.036	−.475
	(.399)	(.525)	(.347)	(.463)
Log(REGROWTH)	−5.154**	−4.520*	−.288	−.292
	(1.964)	(1.988)	(1.199)	(1.220)
Log(INCOMEPC)	−.346	−.497	.675**	.694**
	(.302)	(.315)	(.277)	(.280)
ELFI	.001	.003	.005	.004
	(.008)	(.008)	(.007)	(.007)
CATHPOP	−.004	.001	−.014#	−.013
	(.011)	(.012)	(.008)	(.008)
MOSLEMPOP	.002	.003	−.003	−.003
	(.008)	(.008)	(.007)	(.007)
PRESIDENTIAL	.817	.775	—	—
	(.598)	(.592)		
MILREGIME	—	—	1.548***	1.621***
			(.359)	(.368)
Log(REGIMEDURATION)	.253	.221	−.282	−.296
	(.355)	(.361)	(.213)	(.220)
Log(DSYEARS)	−.399	−.518	.087	.118
	(.398)	(.403)	(.190)	(.189)
TRADEDEPENDENCE	−.016#	−.014	−.018#	−.016#
	(.009)	(.009)	(.009)	(.010)
REGIONDEM	−5.014**	−4.229*	4.514**	3.846*
	(1.977)	(2.014)	(1.628)	(1.643)
YEAR	−.058*	−.066**	.013	.024
	(.027)	(.026)	(.023)	(.025)
Log(INFLATION)*YEAR	—	−.092*	—	.108*
		(.046)		(.047)
N_1, N_0	39,534	39,534	41,1504	41,1504
−2 log likelihood	225.646	220.664	322.583	317.493
Concordant/discordant predictions (%)	83.7/15.8	85.4/14.2	79.4/18.7	80.4/17.8

#$p \leq .10$.
*$p \leq .05$.
**$p \leq .01$.
***$p \leq .001$.

Equation 2 shows that the coefficient of log(INFLATION) in model 2 is really .917 − .092*YEAR. Evaluating this expression at different values of YEAR gives the "simple slope" coefficient of log(INFLATION) at each value. These simple slopes decrease as YEAR increases, indicating that the positive effect of high inflation on the likelihood of breakdown decreased over time. The standard errors of these simple slopes are given by:

$$SE_{YEAR} = \sqrt{C_1 + 2*YEAR*C_2 + YEAR^2*C_3}, \qquad (3)$$

where C_1 and C_3 are the variances of the coefficients of log(INFLATION) and log(INFLATION)*YEAR in model 2 and C_2 is their covariance (Aiken and West 1991, 14–16). The *t*-statistics calculated from the simple slopes and their standard errors indicate that these simple slopes are significant at the .05 level for values of YEAR from 1950 through 1972 and at the .10 level in 1973 and 1974. High inflation therefore had a significantly positive effect on the likelihood of breakdown until 1972, a marginally positive effect during the next two years, and no effect thereafter.

We can infer from the literature reviewed that democratic transition is affected by all of the variables listed except PRESIDENTIAL, because executive-system type presumably has little effect on democratization under nondemocratic and semidemocratic regimes. As in the analysis of breakdown, there is no reason to assume that a hierarchical causal structure exists among these variables, except for the possibility that the economic crisis variables interact with some of the other variables. I therefore began my analysis of democratic transition with model 3 of Table 2, which includes all variables listed except PRESIDENTIAL. The coefficients of log(INFLATION) and log(REGROWTH) are both insignificant in model 3, indicating that high inflation and slow or negative growth do not independently affect democratic transition.

I again examined the interactions of log(INFLATION) and log(REGROWTH) with the structural variables and YEAR by adding the corresponding interaction terms separately to model 3. The only ones that were significant were log(INFLATION)*REGIONDEM and log(INFLATION)*YEAR. When I added both of these interaction terms to model 3, the significance of the first fell sharply; I therefore dropped it from the model, producing model 4. The significantly positive coefficient of log(INFLATION)*YEAR in model 4 indicates that high inflation reduced the likelihood of democratic transition in the early part of the period under study but not the latter part of this period.

Several structural variables are also significant in model 4. The coefficients of log(INCOMEPC), MILREGIME, and REGIONDEM are all positive and significant, indicating that democratic transition is more likely to occur in Third World countries that are relatively developed, have a high proportion of democratic neighbors, and have military rather than civilian regimes. The coefficient of TRADEDEPENDENCE is negative and significant only at the .09 level, indicating that high trade dependence may perhaps reduce the likelihood of democratic transition.

I also applied simple slope analysis to model 4. The coefficient of log(INFLATION) here is −.475 + .103*YEAR. The simple slopes derived from this expression are negative in the early part of the period under study but increase as YEAR increases, becoming positive after 1974. The t-statistics for these simple slopes are significant at the .05 level for years before 1960 and significant at the .10 level before 1969 and after 1986, indicating that high inflation had a negative effect on the likelihood of democratic transition in the 1950s, a marginally negative effect throughout the early and mid-1960s, and a marginally *positive* effect on the likelihood of transition in the late 1980s.

Except for the intercept term, each coefficient appearing in both models that is significant in model 2 has the opposite sign or is not significant in model 4, and vice versa. This suggests that the causal model of democratic breakdown embodied in model 2 is fundamentally different from the causal model of democratic transition embodied in model 4. Moreover, four of the five variables that appear in both models and have significant coefficients in one model have coefficients with the opposite sign in the other. This suggests that these four variables may each increase the likelihood of regime change in one direction and reduce it in the other, in a complementary manner.

We can examine these findings more precisely by using the described procedures to test whether the coefficients in model 2 differ significantly from the corresponding coefficients in model 4 and their inverses. Because MILREGIME does not appear in model 2 and is equal to 0 for all observations in the data set used to estimate this model, and because PRESIDENTIAL does not appear in model 4, I modified these procedures in two minor ways to carry out these tests. First, since the two models must have identical sets of explanatory variables, I included both MILREGIME and PRESIDENTIAL in the pooled models. Second, since MILREGIME is equal to 0 in the sample used to estimate model 2, it is perfectly correlated with the product of itself and the dummy variable that distinguishes the two samples. I therefore dropped the intercept term and this dummy variable and switched the 0 and 1 values of MILREGIME before estimating the pooled models. These models are shown in Table 3.

Models 2m and 4m in Table 3 are the modified versions of models 2 and 4 that are compared in these pooled models. Model 2m contains no intercept term and includes MILREGIME. Since the values of MILREGIME have all been switched from 0 to 1, its coefficient in model 2m is identical to that of the intercept term in model 2. All other coefficients in model 2m are identical to the corresponding coefficients in model 2. In model 4m I have added PRESIDENTIAL to model 4. The coefficient of PRESIDENTIAL is not significant in model 4m. The coefficient of CATHPOP changed signs and became marginally significant at the .09 level. (Its significance level in model 4 was .11.) All other coefficients in model 4m are similar to the corresponding coefficients in model 4, implying that the addition of PRESIDENTIAL did not substantially affect this model.

The coefficients in the "difference model" and "sum model" in Table 3 are equal to the differences and sums, respectively, of the corresponding coefficients in models 2m and 4m. Their significance levels test whether one coefficient in each pair differs from the other and the inverse of the other, respectively. The difference model indicates that the coefficients of log(INCOMEPC) and REGIONDEM in models 2m and 4m differ significantly from one another and those of log(REGROWTH) and MILREGIME differ marginally from one another. However, since MILREGIME is really the intercept term in model 2m, its coefficient in the difference model is meaningless. Using the methods described, we can calculate the simple slopes, standard errors, and t-statistics of log(INFLATION) for different values of YEAR in the difference model. Doing so reveals that the simple slopes of log(INFLATION) in models 2m and 4m differ significantly at the .05 level before 1975 and at the .10 level before 1977. Thus all variables except log(REGROWTH) that appear in both models and are significant in one or the other have coefficients that differ significantly from one

TABLE 3

Tests of Coefficient Differences and Sums

EXPLANATORY VARIABLE	MODEL 2M	MODEL 4M	DIFFERENCE MODEL	SUM MODEL
Intercept	—	−2.763*** (.576)	—	—
Log(INFLATION)	.917* (.525)	−.486 (.466)	1.403* (.702)	.431 (.702)
Log(REGROWTH)	−4.530* (1.988)	−.295 (1.206)	−4.235# (2.325)	−4.825* (2.325)
Log(INCOMEPC)	−.497 (.315)	.760** (.300)	−1.258** (.435)	.263 (.435)
ELFI	.003 (.008)	.005 (.007)	−.002 (.011)	.008 (.011)
CATHPOP	.001 (.012)	−.016# (.009)	.017 (.015)	−.015 (.015)
MOSLEMPOP	.003 (.008)	−.004 (.007)	.007 (.011)	−.001 (.011)
PRESIDENTIAL	.775 (.592)	.421 (.659)	.354 (.886)	−.354 (.886)
MILREGIME	−3.295*** (.314)	−1.989** (.689)	−1.305# (.758)	−5.284*** (.758)
Log(REGIMEDURATION)	.221 (.361)	−.264 (.226)	.489 (.425)	−.048 (.425)
Log(DSYEARS)	−.518 (.403)	.119 (.189)	−.636 (.445)	−.399 (.445)
TRADEDEPENDENCE	−.014 (.009)	−.016# (.010)	.002 (.014)	−.031* (.014)
REGIONDEM	−4.227* (2.014)	3.989* (1.650)	−8.198** (2.604)	−.260 (2.604)
YEAR	−.066** (.026)	.027 (.026)	−.089* (.037)	−.046 (.037)
Log(INFLATION)*YEAR	−.092* (.046)	.100* (.047)	−.192** (.066)	.008 (.066)
N_1, N_0	39,534	41,1504	80,2038	80,2038
−2 log likelihood	220.664	317.066	537.730	537.730
Concordant/discordant predictions (%)	85.4/14.2	80.5/17.6	84.4/14.5	84.4/14.5

$^{\#}p \leq .10$.
$^{*}p \leq .05$.
$^{**}p \leq .01$.
$^{***}p \leq .001$.

another in the two models, and the coefficients of log(REGROWTH) differ marginally from one another. The only variables that are marginally significant in either model (CATHPOP and TRADEDEPENDENCE, which are both significant only at the .09 level in model 4m) have coefficients that are not significantly different in the two models.

The sum model indicates that the coefficients of log(REGROWTH), MILREGIME, and TRADEDEPENDENCE in model 2m differ significantly from the inverses of their coefficients in model 4m. The negative coefficient of log(REGROWTH) in the sum model indicates that slow or negative economic growth does not reduce the likelihood of democratic transition by an amount comparable to its positive effect on the likelihood of breakdown. As in the difference model, the coefficient of MILREGIME in the sum model is not meaningful. The negative coefficient of TRADEDEPENDENCE in the sum model indicates that trade dependence does not have a positive effect on the likelihood of breakdown that is comparable to its marginally negative effect on the likelihood of transition.

None of the other coefficients in the sum model are significant. Thus, for all other variables that are significant in either model 2m or model 4m, the positive effect on regime change in one direction is not significantly different in magnitude from its negative effect on regime change in the other direction. For example, if log(INCOMEPC) increases by one unit and the other variables remain constant, the log(odds) of a democratic breakdown occurring will decrease by .497 and that of a democratic transition occurring will increase by .760, according to models 2m and 4m; the sum model indicates that these amounts are not significantly different from one another. Thus an increase in per capita income at any given level has *complementary* effects on democratization in the two contexts embodied in models 2m and 4m: it reduces the likelihood of breakdown in contexts where this can occur by roughly the same amount that it increases the likelihood of democratic transition in contexts where this can occur. The same is true of REGIONDEM; and simple slope analysis indicates that it is also true of log(INFLATION) for all values of YEAR from 1950 through 1989.

The difference and sum models therefore indicate that the causal processes affecting breakdown and transition are fundamentally different yet also quite complementary, in three important ways: (1) high inflation in the early part of the period under study increased the likelihood of breakdown and reduced the likelihood of transition by comparable amounts, while in the late 1980s it marginally increased the likelihood of transition by an amount comparable to its (insignificantly) negative effect on breakdown; (2) high levels of wealth and democratic neighbors both increased the likelihood of breakdown by amounts comparable to their (marginally or significantly) negative effects on transition; and (3) Catholicism marginally reduced the likelihood of transition by an amount comparable to its (insignificantly) positive effect on breakdown. However, these two causal processes are only marginally different in another way: slow or negative economic growth increased the likelihood of breakdown by an amount that was only marginally greater than its (insignificantly) positive effect on transition. Finally, these two processes were very similar in a rather trivial way: the marginally negative effect of trade dependence on the likelihood of transition (which was significant at only .09) was comparable in magnitude to its (insignificantly) negative effect on breakdown.

SUMMARY AND DISCUSSION

The foregoing empirical analysis enables us to evaluate the six hypotheses. Hypothesis 1 stated that economic crises trigger democratic breakdown. The empirical analysis provides strong support for this hypothesis: high inflation increased the likelihood of breakdown from 1950 until the mid-1970s (though not thereafter), and slow or negative economic growth increased the likelihood of breakdown throughout the period under study (1950–89).

Hypothesis 2 stated that economic crises trigger democratic transition in a manner that is similar to their effect on breakdown, while hypothesis 3 stated that they inhibit transition in a manner that complements their effect on breakdown. The empirical evidence does not really support either of these hypotheses. High inflation significantly or marginally reduced the likelihood of transition in the 1950s and 1960s, and the sum model in Table 3 indicates that this effect was complementary to the positive effect of inflation on breakdown in this period in the sense that the corresponding coefficients were comparable in magnitude. This finding contradicts hypothesis 2 and conforms with hypothesis 3. However, high inflation marginally *increased* the likelihood of transition in the late 1980s, and the sum model indicates that this marginally positive effect was comparable in magnitude to the (insignificantly) negative effect of inflation on breakdown in this period. This finding contradicts both hypotheses. Slow or negative economic growth did not significantly affect the likelihood of transition during 1950–89, and the difference and sum models in Table 3 indicate that it had neither complementary nor clearly similar effects on the two types of regime change. This finding also contradicts both hypotheses.

We can conclude from these findings that economic crises do not simply undermine the legitimacy of whatever type of regime currently exists in a country, thus triggering regime change in either direction, as argued by several authors. However, we must also conclude that while inflationary crises inhibited democratization in both contexts in a complementary manner in the 1950s and 1960s—the period about which O'Donnell (1973) and others who made this argument were writing—they may actually have *facilitated* democratic transition in the late 1980s; and recessionary crises did not have this kind of complementary effect throughout 1950–89. Thus economic crises are not necessarily incompatible with democracy, as implied by these writers.

Hypothesis 4 provides a better explanation of these findings, though one that is also not entirely satisfactory. This hypothesis stated that economic crises no longer triggered breakdown but began to trigger democratic transition in the 1980s. The two sets of inflation findings not only conform with this hypothesis but take it a step further: inflationary crises inhibited democratization *in both contexts* in the 1950s and 1960s, and they marginally increased the likelihood of transition in the late 1980s by an amount comparable to their (insignificantly) negative effect on breakdown. Thus inflationary crises have neither similar nor uniformly complementary effects on the two types of regime change. Rather, their effects have changed dramatically over time: they inhibited democratization in the 1950s and 1960s but seem to have facilitated democratization in the late 1980s. Recessionary crises, however, did not have time-varying effects of this sort and therefore contradict hypothesis 4.

The findings regarding inflation clearly support the arguments of Remmer and Huntington that the processes affecting democratization were very different in the 1980s than in earlier eras.[12] Several cautionary points should be made, however. First, Remmer and Huntington do not provide any clues that might explain why recessionary crises did not have time-varying effects like those observed for inflationary crises. Second, this finding does not address the underlying factors cited by Remmer as causes of the time-varying effects of economic crises, and it certainly does not address the much more general arguments about the third wave of democratization made by Huntington. Finally, while the time-varying effects of inflation observed above hold through 1989, there is no particular reason to believe they hold in subsequent years. This finding therefore does not allow us to judge whether the changing effects of economic conditions on democratization discussed by Remmer and Huntington are *permanent* (as might be inferred from their work) or *cyclical*, as argued by Malloy (1987).

Hypothesis 5 stated that the structural factors discussed in the literature reviewed affect democratic breakdown and transition in the ways indicated, each in a complementary manner. Of the factors I could incorporate into the analysis, several affected breakdown and transition in the expected ways: both high income per capita and military regimes increased the likelihood of democratic transition; countries with many democratic neighbors were less likely to experience breakdown and more likely to experience transition; and very weak evidence emerged that Catholic countries and those with trade-dependent economies might perhaps be less likely to experience transition. The sum model indicates that income per capita and democratic neighbors have complementary effects on democratization in both contexts, increasing the likelihood of transition by amounts not significantly different from their negative effects on breakdown. Catholicism also seems to have complementary effects, very weakly reducing the likelihood of transition by an amount comparable to its (insignificantly) positive effect on breakdown. Trade dependence, however, does not have complementary effects in the two contexts, and its very weak, negative effect on transition is similar in magnitude to its (insignificantly) negative effect on breakdown. Since military regimes cannot experience democratic breakdown, their effect on democratization is also necessarily noncomplementary.

Although not the main focus of this study, several of these findings are noteworthy. The income per capita finding confirms once again the importance of development-related socioeconomic conditions of the sort emphasized by modernization theorists in facilitating democracy. The strong positive effect of military regimes on democratic transition indicates that there are important differences in the durability of military-led and civilian-led nondemocratic regimes, implying that military rule generally has an exceptional, temporary character. This is a novel finding, inasmuch as the literature on democratic transition has paid little attention to these differences. The finding that a high proportion of democratic neighbors facilitates democratization in both contexts indicates that the emphasis placed on "demonstration effects" in much of the recent literature is well founded and suggests that other international political factors (e.g., pressure from the developed countries and human rights organizations) may also facilitate democratization.

It is also noteworthy that several widely discussed structural factors had little or no apparent effect on breakdown and transition. Most surprisingly, the presidential-system variable and the two measures of institutionalization had no effect in either direction, suggesting that the recent emphasis on the role of political institutions in democratization may have been misguided. The two religion-based political culture measures also had little or no effect, suggesting that the religious denomination (although not necessarily political culture in general) may have been overemphasized in the democracy literature as well.

Finally, hypothesis 6 stated that economic crises trigger breakdown and possibly also transition in conjunction with some of the structural factors previously discussed. The only interaction terms representing conjunctural effects that were significant were those embodying the time-varying effects of inflation. These findings imply that inflation acts in conjunction with time-varying factors of some sort—perhaps those discussed by Remmer and Huntington—but they do not indicate what these time-varying conjunctural factors are. No evidence emerged that economic crises trigger regime change in conjunction with the examined structural factors that tended to increase during the period under study: per capita income, institutionalization, prior democratic experience, and the prevalence of democratic neighbors. Also, no evidence emerged that economic crises are especially likely to trigger breakdown in countries with a relatively high level of development (as argued by O'Donnell and others) or in countries that are ethnically heterogeneous or have presidential systems (as implied by Linz).

CONCLUSION

Although these findings are intriguing, we must bear in mind that several of the factors discussed that are widely thought to affect regime change were not included as explanatory variables in the analysis, and some of the factors that were included may not have been adequately measured. The most glaring omission is the process-oriented factors discussed in much of the recent literature on democratic transition, which cannot easily be incorporated into a research design of the type used here. The determinants of regime change identified in this study therefore should not be considered an exhaustive list. Moreover, despite my efforts to include a large number of control variables in the models of regime change, the omission of important explanatory variables raises the problem of specification error in theses models, implying that the causal relationships they depict may not have been accurately estimated.

It is also important to emphasize that a time-series cross-national study of this sort necessarily focuses on broad, systematic causal processes that hold across time and space rather than more idiosyncratic causal processes that hold only at certain times or in certain countries or regions—processes that can be equally momentous for the societies affected. In particular, the presented evidence that economic crises significantly affected regime change in certain ways at certain times does not imply that these crises never affected regime change in other ways or at other times; it simply implies that they did not do so *systematically*.

This study has a number of important implications for further research on regime change. First, the finding that economic crises systematically affect regime change raises some intriguing questions: Why did the effect of inflationary crises change over time while that of recessionary crises did not? Do underlying structural conditions that cause economic crises (e.g., the need to "deepen" the economy during ISI) also systematically affect regime change? Second, the finding that inflation had time-varying effects raises the question of whether other factors also have time-varying effects, as implied by Huntington (1991). Third, the findings that military regimes and democratic neighbors facilitate democratization are quite novel and warrant further study: Why do military regimes have such a strong impact? Do other kinds of "demonstration effect" also affect regime change?

Finally, this study has several broader research implications. Together with other studies that use similar statistical methods (Berry and Berry 1990; Bienen and Van de Walle 1989; Hanneman and Steinback 1990; Hannon and Carroll 1981; Londregan and Poole 1990), this study demonstrates that event history analysis can be a very useful approach for studying processes of discrete political change. This implies not only that the statistical techniques employed in these studies can be very useful but also that research designs and data sets focusing on the occurrence of discrete events within their historical contexts can be valuable in studying political change. This study also demonstrates that time-varying, synchronic factors such as economic crises and interaction-effect models that enable us to examine the changing impact of these factors can be very useful in studying political change.

APPENDIX

Democratic-to-Semidemocratic Regime Changes. Turkey, 1957; Sierra Leone, 1968; Sri Lanka, 1983.

Democratic-to-Nondemocratic Regime Changes. Guatemala, 1954; Ghana, 1960; South Korea, 1961; Burma, 1962; Dominican Republic, 1963; Congo, 1963; Kenya, 1966; Togo, 1967; Sierra Leone, 1967; Turkey 1971; Madagascar, 1971; Ghana, 1972; Philippines, 1972; Chile, 1973; Uruguay, 1973; India, 1975; Thailand, 1976; Turkey, 1980; Upper Volta, 1980; Ghana, 1982.

Semidemocratic-to-Democratic Regime Changes. Philippines, 1953; Ecuador, 1984; Turkey, 1987.

Semidemocratic-to-Nondemocratic Regime Changes. Honduras, 1954; Pakistan, 1958; Turkey, 1960; Peru, 1962; Honduras, 1963; Ecuador, 1963; Nigeria, 1966; Peru, 1968; Gabon, 1968; Ecuador, 1970; Zambia, 1971; Sierra Leone, 1973; Upper Volta, 1974; Pakistan, 1977; Kenya, 1982; Nigeria, 1983.

Nondemocratic-to-Democratic Regime Changes. Colombia, 1958; Venezuela, 1959; South Korea, 1960; Turkey, 1961; Dominican Republic, 1963; Sierra Leone, 1968; Ghana, 1969; Turkey, 1974; Thailand, 1975; India, 1977; Dominican Republic, 1978; Upper Volta, 1978; Ghana, 1979; Peru, 1980; Bolivia, 1982; Argentina, 1983; Brazil, 1985; Uruguay, 1985; Phillipines, 1986; Pakistan, 1988; South Korea, 1988.

Nondemocratic-to-Semidemocratic Regime Changes. Honduras, 1957; Peru, 1963; Gabon, 1964; Ecuador, 1968; Upper Volta, 1971; Pakistan, 1972; Senegal, 1978; Ecuador, 1979; Nigeria, 1979; Kenya, 1979; Honduras, 1982; Turkey, 1983; Guatemala, 1986; Sudan, 1986; Thailand, 1986; Panama, 1989; Paraguay, 1989.

Notes

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1. An important cross-national, time-series data set containing measures of regime type and regime change is Gurr's (1990) *Polity II* data set, which includes 10-point, ordinal measures of democracy and autocracy and a variable called *polity change*, which does identify distinct instances of political change. Like Bollen's measure, Gurr's democracy and autocracy measures do not permit us unambiguously to identify distinct instances of regime change. Gurr's polity change measure does not focus only on democratic/autocratic changes; indeed, in many cases his democracy and autocracy measures do not change when polity change occurs. Consequently, this data set is not suitable for my purposes.

2. The data set described here is a simplified version of the one discussed in Gasiorowski (1990), which I never completed.

3. The data set also contains a fourth regime category, transitional regimes, defined as those in which deliberate and ultimately successful efforts are being made by, or with the clear acquiescence of, top government officials to engineer a change from one of these three regime types to another. To keep the analysis from becoming too complex I have ignored transitional regimes here, treating them merely as continuations of the regimes that preceded them.

4. The countries and years in which "modern" states were established are Afghanistan, 1747; Bhutan, 1907; China, 1911; Ethiopia, 1871; Iran, 1906; Liberia, 1847; Nepal, 1767; Thailand, 1932; and Turkey, 1921. None of these countries were formally colonized. My judgments about the establishment of "modern" states are based on my reading of the historical record.

5. The most important sources I used were the case studies contained in Diamond, Linz, and Lipset (1990, vols. 2–4). I also relied heavily on Europa Publications' *Political Handbook of the World* and *Keesing's Record of World Events*. I occasionally made use of *Africa Contemporary Record*, Wiarda and Kline (1985), Long and Reich (1986), Wiseman (1990), and monographs for certain countries.

6. For most countries I constructed these measures from the consumer price index (CPI) and real gross domestic product (GDP), obtained from a tape version of International Monetary Fund (1988), updated wherever possible with data from International Monetary Fund (1993). Because adequate CPI date were not available for Nicaragua, Bolivia, Chile, Argentina, Uruguay, Uganda, Botswana, Libya, and Indonesia, I used the GDP deflator as my inflation measure for these countries. Similarly, for Senegal, Niger, Ivory Coast, Burkina Faso, Togo, Cameroon, Gabon, Congo, Kenya, Rwanda, South Africa, Lesotho, Madagascar, Algeria, Sudan, Turkey, Egypt, and the Philippines I used current-value GDP deflated with the CPI as my real economic growth measure.

7. INCOMEPC is real GDP per capita adjusted with a Chain index to reflect international prices, from Summers and Heston (1991). ELFI, CATHPOP, and MOSLEMPOP are from

Taylor and Hudson (1972). This source reports data for only a single year for these three variables. Since these variables change very slowly, I use these values for all years covered by the dataset (1950–1989). PRESIDENTIAL and MILREGIME are from the "effective executive" variable in a tape version of Banks (1979), which contains data through 1988. Since Banks apparently reports the year-end value of this variable, I use its one-year lag, giving me the values of these variables for the first day of the current year. REGIMEDURATION is the number of years since the last regime change, calculated from my regime change data. DSYEARS is the number of prior years during which the country had a democratic or semidemocratic regime, also calculated from my data set. TRADEDEPENDENCE is the variable OPEN, from Summers and Heston (1991). REGIONDEM is the average proportion of months in the current year in which countries in the region were democratic, calculated from my data set. The regions covered are Latin America, the Middle East and North Africa, Subsaharan Africa, South Asia (which runs from Pakistan and Afghanistan through Myanmar), and East Asia.

8. The smallest values of these five variables appearing in the data set were −18.2, −35.6, 212, 0, and 0, respectively. Since the natural logs of zero and negative numbers do not exist and the logs of numbers between zero and one are highly skewed in comparison with those of larger numbers, I added 20, 37, −211, 1, and 1, respectively, to these variables before taking their logs. For a discussion of this practice in a related context, see Dixon, Muller, and Seligson (1993, 985).

9. The countries and time periods included are Algeria, 1971–88; Argentina, 1977–89; Benin, 1972–86; Bolivia, 1966–89; Brazil, 1966–89; Burkina Faso, 1961–86; Burundi, 1972–89; Cameroon, 1972–89; Central African Republic, 1967–89; Chile, 1963–89; China, 1968–89; Colombia, 1952–89; Congo, 1961–89; Costa Rica, 1962–89; Dominican Republic, 1952–89; Ecuador, 1953–89; Egypt, 1954–89; El Salvador, 1953–89; Ethiopia, 1967–86; Gabon, 1964–89; Ghana, 1958–89; Guatemala, 1952–89; Haiti, 1960–89; Honduras, 1952–89; India, 1962–89; Indonesia, 1962–89; Iran, 1961–89; Israel, 1953–89; Ivory Coast, 1962–86; Jamaica, 1963–89; Jordan, 1971–89; Kenya, 1964–89; Kuwait, 1985–88; Lesotho, 1975–89; Liberia, 1967–86; Madagascar, 1966–89; Malawi, 1970–89; Malaysia, 1972–89; Mauritania, 1972–89; Mexico, 1950–89; Morocco, 1959–89; Myanmar, 1952–89; Nepal, 1966–86; Nicaragua, 1962–87; Niger, 1965–89; Nigeria, 1961–89; Pakistan, 1955–89; Panama, 1952–89; Papua New Guinea, 1976–89; Paraguay, 1952–89; Peru, 1962–89; Philippines, 1951–89; Rwanda, 1968–89; Saudi Arabia, 1985–89; Senegal, 1969–89; Sierra Leone, 1965–89; Singapore, 1966–89; South Africa, 1950–89; South Korea, 1955–89; Sri Lanka, 1952–89; Sudan, 1971–89; Syria, 1961–89; Tanzania, 1967–88; Thailand, 1955–89; Togo, 1965–87; Trinidad, 1963–89; Tunisia, 1962–89; Turkey, 1955–89; Uganda, 1983–89; Uruguay, 1967–89; Venezuela, 1952–89; Yemen Arab Republic, 1974–84; Zaire, 1965–89; Zambia, 1965–89; Zimbabwe 1981–89.

10. The likelihood or odds of an event occurring is equal to the probability of the event (P) divided by one minus this probability, that is, odds $= P/(1 - P)$.

11. I estimated all of the logit models reported here with PROC LOGISTIC in SAS release 6.08. SAS inexplicably treats 0 values as events and 1 values as nonevents, so I have reversed the signs of all coefficients reported here. Note that the percentages of concordant and discordant predictions do not add to 100 because some observations usually predict "ties."

12. It is worth noting that the years when high inflation ceased to have a marginally positive effect on breakdown (1974) and a marginally negative effect on transition (1968) correspond closely to the date Huntington identifies as the beginning of the "third wave": 25 April 1974, when a democratic transition was initiated in Portugal (1991: 3).

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NATURE OR NURTURE? SOURCES OF FIRM PREFERENCE FOR NATIONAL HEALTH REFORM

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I shall explore the process by which firms develop their political preferences, using the case of national health reform. Although rising health costs have heavily burdened many companies, I argue that economic interests alone are unable to account for the variation in firm response to the national reform effort. Rather, institutional factors, shown elsewhere to shape government decision making, also influence corporate preferences. These are (1) the institutionalization of private policy expertise within the firm, (2) firm participation in policy groups, and (3) policy legacies. These findings challenge conventional views of business political mobilization that suggest largely autonomous agents acting on the basis of easily recognized self-interests. Preference formation and corporate mobilization transpire in collective settings as a new stratum of corporate policy managers search for solutions to social problems. The primacy of economic concerns is very real. But institutional analysis explains how these economic concerns are interpreted and acted upon.

Business influence over public policy in general and social policy in particular is hotly contested among political scientists. Yet the opposing camps in both debates are in fundamental agreement about the source of corporate preferences. In the corporate power debate, one side vests the business class with enormous influence, either through its instrumental control of positions of authority or through its threats associated with the state's structural dependence on corporate investment (Lindblom 1977; see also Przeworski and Wallerstein 1988, 11–30; Quinn and Shapiro 1991, 851–74; Swank 1992, 38–54).[1] The other side suggests that business acts as another interest group in the political struggle for government benefits (Truman 1981). Yet both sides agree that profits drive corporate preferences, interests in policy are easily identified, and the material conditions of the firm or industry largely predict corporate political positions.

The literature on welfare state development is similarly both divided and unified. Much of the cross-national comparative literature assumes that business is important to social innovation only in its ability to impede progress. Countries with strong labor movements that can challenge corporate domination have higher levels of welfare provision (Korpi 1980, 296–315; Stephens 1979). Governments with a high degree of state capacity, where government bureaucrats enjoy a fair amount of autonomy from the pressures of private interests, also have more developed social policy (Weir, Orloff, and Skocpol 1988). Those writing in the corporate liberal tradition disagree, seeing the heavy hand of business in most welfare initiatives. Corporate liberals perceive that social programs are functional to the long-term survival of the capitalist system, even if these programs impose short-term costs on individual businessmen (Berkowitz and McQuaid 1992; Kolko 1977; Weinstein 1968; see also Myles 1989; Quadagno 1988). Again, both assume that business managers act on materially grounded interests but disagree over the corporate capacity to recognize the long-term relationship between growth and social welfare.

The intense polarization over whether firms participate in social initiatives suggests that the truth lies somewhere in between: business supports social policies some of the time. The welfare-state theorists are too quick to dismiss cross-class coalitions or conditions in which government actors mobilize corporate allies (C. Martin 1991). The corporate liberal theorists assume an excessively deterministic relationship between the level of industrialism and the expression of corporate preferences, paying little attention to the mechanisms by which corporate liberals determine their interests. After all, if business managers automatically grasp their interests in social programs functional to growth, one suspects that social spending in the United States would be much higher. Corporate preferences may be more fluid than either side admits; it is important to look for the source of this fluidity. Business managers must be brought to understand the connections between growth and social policies; the task for social scientists is to understand how this happens.

The central problem here is to explain how firms develop their preferences for social policy initiatives and especially to understand the mechanisms by which new social policy paradigms win corporate converts. What are the conditions under which some companies sign on to far-reaching government solutions when others reject them? The analysis is based on a statistical analysis of business support for employer mandates, a critical and contentious component of the national health reform package.

I found that although material concerns were the underlying force for health reform, institutional factors were critical to business acceptance of employer mandates and were at least as responsible for firm variations in policy preference as the economic structural characteristics of the company. Business acceptance of mandates depended on the development of *corporate policy capacity* in the health area, or the ability of business managers to grasp complicated social issues and to act in support of this social agenda. Corporate policy capacity varied across companies, and was related to three institutional characteristics of the firm: internal capacity for policy evaluation, participation in external networks, and policy legacies.

First, firm positions reflected the internal policy expertise within the companies, or the firms' capacity for policy evaluation. Many U.S. companies established government affairs offices in Washington in the 1970s in order to oppose government regulation; yet over time these offices became centers of policy expertise (Vogel 1989). A high level of policy expertise within the firm led to greater technical awareness of the issue, providing a counterweight to the refuge of ideology within which business managers often intuitively greet social concerns. Firms in this study that possessed such offices were much more likely to develop positions in favor of employer mandates.

Second, firm preferences reflected the degree to which policy experts within the company were connected to external networks. Groups that focused attention on social concerns and brought participants into contact with actors in state and labor helped business managers to develop informed and more sympathetic positions on social issues. Benefits and human resource professionals within companies supporting health reform belonged to more business groups than those opposing government intervention. Participants in reform-oriented companies credited these groups with transformational experiences in their thinking about health policy issues.

Third, to a lesser extent, firms' current preferences for reform reflected previous attempts to control costs: the concept of policy legacies in the institutional literature has an analog in strategy legacies at the firm level (Pierson 1993; Weir 1992). Business leaders, like state actors, "puzzled" over appropriate strategies as well as "powered" (Heclo 1974). Policy professionals in the proreform companies "tried everything" to curb costs at the firm level and finally in desperation moved on to supporting a government solution. Acceptance of mandates evolved through a process of social learning.

Thus, support for health reform in the business community was made possible by the development of a stratum of policy professionals within firms who understood the political game and worked to put political issues on the business agenda. Sharing their experiences collectively in professional groups and forums, these private experts awoke to the enormous scope of the health financing problem and worked to educate top management within their firms. Growing weary of failed market interventions, they embraced state solutions.

This emerging political stratum may ultimately transform the modern corporation and parallel the split between capital and management in the earlier part of the twentieth century. These policy professionals have been coopted by government associates, by the labor groups they negotiate with, and by the literature and truths of their profession. They are quietly undermining the earlier capitalist ideology and represent a potential for building prostate sentiment in the business community.

ECONOMIC THEORIES OF FIRM PREFERENCE

Business managers have had standing to worry about health care. Employer health care costs increased dramatically throughout the 1980s, jumping from 2.2% of salaries and wages in 1965 to 8.3% by 1989 (Levit et al. 1991, 127–29). The system failed to guarantee universal access, leaving 34 to 37 million Americans uninsured. Large companies providing benefits have subsidized the health expenses of the uninsured, of other companies, and of the government (Amkraut 1987, 50).[2]

President Clinton proposed to establish universal access and to reduce cost shifting with a mandate requiring all employers to pay 80% of insurance premiums for all employees (U.S., Department of Commerce, National Technical Information Service 1993). The employer mandate was a major point of contention during the legislative debate, because the president identified universal access as a nonnegotiable component in any health reform and small business pledged to opposed any legislation with an employer mandate. Critical to financing the plan and focal in legislative debate, the employer mandate has merited the attention of anyone interested in corporate support for social regulation.

Concerns about their own health costs and about the long-term viability of the system motivated many large employers to support employer mandates. Yet others in the business community rejected this approach, fearing government involvement on ideological grounds, preferring market solutions at the firm or community levels, or choosing to shift costs to their workers. For example, all of the companies in this study provided benefits, but a quarter of the respondents firmly opposed employer mandates. Thus it is important to understand how companies decided whether to endorse employer mandates.

The question of preference formation has inspired generations of social scientists. Answers gravitated toward two broad poles of social theory: the economic and the institutional (Hirschman 1977). Economic explanations come from a variety of traditions but have converged on four assumptions about corporate political action. First, competitive strategies and political positions can be inferred from the economic structure of the firm and the industry in which it is located. Businessmen are primarily concerned about profitability; therefore, to understand preference, we must examine material circumstance (Frieden 1988; Gourevitch 1986; Kurth 1979; Porter 1980; Rogowski 1989; Salamon and Siegfried 1974, 1026–43).

Second, firms will usually resist all government interventions that interfere with their profitability, especially social redistributive policies (Jacobs 1988, 852–81). Since businessmen control investment, government will shy away from policies that might trigger a loss of business confidence, resulting in disinvestment.

Third, there are a few situations in which firms might be induced to support social policies that impose costs for collective concerns on firms. Large, monopoly-capital companies may choose to appease working-class demands rather than risk labor strife (Ferguson 1984, 41–94). Since regulation may affect firms within industries differently, public policy can alter these competitive positions (Gals and Buchholz 1987, 31–41; Harris 1985, 16). If the companies already assume social costs, they may want their competitors (or the state) to absorb these costs (Baldwin 1990; Gordon 1991, 165–207; Weinstein 1968).

Fourth, political mobilization is a decision made by rational individuals who calculate the costs and benefits of such action. Although acknowledging the difficulties in collective action, the economic view suggests that businessmen have an easier time organizing because they can often also gratify self-interests (Olson 1965).[3] Businessmen can rely on an "individualist and purely instrumental form of collective action," whereas for workers, "interests can only be met to the extent they are partially redefined" (Offe 1985, 183–84).

This description is drawn somewhat sharply for purposes of analytic clarity. In particular, it does injustice to the very important insights of those writing in the pluralist tradition. Bauer, Poole, and Dexter (1972) and Berry (1977) acknowledge that corporate structure influences a company's policy position. Vogel pays considerable attention to business mobilization, although he assumes that the impetus for mobilization comes from threats outside the business community (Vogel 1989). But pluralists fail to theorize adequately the connections between institutional factors and outcomes; in particular, they have underemphasized the cognitive and interpretive benefits of organization.

Those with an economic perspective might insist that business support for health reform is simply a matter of easily identified self-interest. In a climate of increased international competition and postindustrial growth, business may need fundamentally different labor market and social welfare strategies. Individual firms, currently offering benefits, may gain substantial economic rewards from policies purporting to benefit the public good. To understand business support for health reform, an economic analyst would identify the winners and losers in the current system and explore how this balance of power might change with national health reform.

Seven economic factors in particular are likely to influence a firm's position on national health reform, producing a series of hypotheses.

HYPOTHESIS 1. *Firms that pay more in health benefits for their employees are more likely to develop a supportive position on health reform.*

The *cost* of health care to a firm should influence a company's decision to take political action in this area. Companies with high health costs may favor mandates to impose costs on their competitors and to rationalize the health system. In addition, they have less to fear from mandates. Firms paying lower health costs may benefit from the perversities in the system by having a competitive advantage from their lower health burden. One might protest, as many economists do, that firms simply shift benefit costs to workers; therefore, the price of health does not matter. Yet in a variety of settings where companies may in fact shift costs, they resist legislation that expands or imposes those costs. Thus firms often oppose corporate taxes, minimum wages, and mandated benefits as a matter of public policy, even if they ultimately do not bear the burden.

HYPOTHESIS 2. *Less profitable companies are more likely to develop a supportive position on health reform.*

It is generally assumed that *less profitable* companies have less ability to bear higher health prices and are more likely to support government costs controls. But profitability could also work in the opposite direction: more profitable companies might better afford to devote resources to public policy and to taking a position. In addition, less profitable companies with limited benefits have reason to fear mandates that demand more expansive provisions.

HYPOTHESIS 3. *Larger firms are more likely to develop a supportive position on health reform.*

Size matters because larger firms are more likely to provide benefits to their workers and may be more willing to avoid labor strife. For example, Hicks and Swank found that national economies with larger firms were more likely to expand welfare state provisions (Hicks and Swank 1984, 81–119).[4] Size is also associated with an institutional effect: larger companies are more likely to have economies of scale in political action (Jacobs 1988, 852–81; Olson 1965; Stigler 1971).

HYPOTHESIS 4. *Unionized companies are more likely to develop a supportive position on health reform.*

Unionization should matter, because high rates of unionization generally indicate rich health benefits and limits to a company's ability to shift costs to workers. Unionized companies used to offer health benefits in lieu of union demands for wage increases, but recently benefits have become a much bigger source of labor strife (Victor 1990, 704, 706). Unions can also help business to develop their views (Bowman 1985, 35–88; Heye 1993; Locke 1990, 347–79). Thus unionized companies should be more likely to support comprehensive reform (1) to force their competitors to also bear health costs, (2) to reduce their commitments to their unions, and (3) to get the state to take over the provision of benefits.

HYPOTHESIS 5. *Capital-intensive firms are more likely to develop a supportive position on health reform.*[5]

Industrial sector theorists usually assume that *capital-intensive firms* are more willing to grant social benefits to workers than their labor-intensive counterparts, because labor costs are a smaller proportion of total production costs. Thus these firms care less about labor costs and can afford to sacrifice higher costs for industrial peace (Ferguson 1984). Labor-intensive firms with low benefits may reject government solutions in favor of cost shifting: mandates might actually increase their health costs.

HYPOTHESIS 6. *Firms in regulated and troubled sectors are more likely to develop a supportive position on health reform.* (Salmon 1987, 1–6).[6]

Two types of industrial sectors are often identified as more likely to accept government intervention: regulated sectors and troubled sectors. Troubled sectors that have recently asked for government subsidies and other types of assistance might be more willing to accept government regulation. (Yet working against this hypothesis is that troubled sectors that do not provide high levels of health benefits may be more fearful of mandates.) Regulated sectors are by definition used to a high level of government intervention and thus might be less cautious about new controls (Burris 1987, 732–44; Handler and Mulkern 1982, 24–29). (Working against their acceptance of mandates is the fact that these sectors have been able to pass costs on to their consumers fairly easily in the past and should be less concerned about health costs. But regulating boards are changing the rules in health care provision, making it much harder for these sectors to pass on costs. Thus they have an additional reason to support government regulation that imposed the same burdens on everyone.

HYPOTHESIS 7. *Firms with a high proportion of exports are more likely to develop a supportive position on health reform.*

Export-oriented firms that must compete with firms from countries with lower health costs might prefer a comprehensive restructuring of the system.

INSTITUTIONAL THEORIES OF FIRM PREFERENCE

Institutionalists hold a very different view of political action, assuming that preferences are somewhat indeterminate and looking at the political and institutional factors that shape preference formation and mobilization.[7] A weak version of this view assumes a gap between material conditions and the articulation of preference; individuals or firms take a while to adjust their positions in response to environmental change (Davies 1962, 5–19). A stronger version is that a range of policies might be in an individual's or company's objective interests; one must decide which position within this range to endorse (Mansbridge 1990; Ziegler 1995). Thus institutionalists accept profits to be a key motivation but argue that profits may be maximized in varying ways. Groups have multiple objectives: the firm should be viewed as a "nonunitary actor" with conflicting and ambiguous interests (Thompson 1982, 233). Even when concrete goals are established, disagreement about policy means to these ends persist, especially when information is limited (Best 1990). As I noted in stating the hypothesis of an economic model, economic characteristics of the firm often have contradictory influences on public policy.

Given such indeterminacy, social context becomes critical to interpreting our situations. Institutionalists doubt that preference is a function of the individual. Rather they believe interests are socially constructed in collective settings (Friedland and Robertson 1990; 32; Moe 1987, 277). Institutions create rules and channel the ideas that help people to interpret their material context and to develop a collective identity (Eckstein 1988, 790–91; Goffman 1974; Granovetter 1985, 481; Levi et al. 1990; Roberts and Bluhm 1981; Scott 1985; Snow et al. 1986, 464–81; Tourraine 1985, 749–88). The institutional context also contributes to the mobilization of interests, in structuring the groups in which people air their grievances and decide to take action (McAdam 1988, 128–36; Plotke 1992, 175–98).

Institutionalists have largely focused on cross-national differences in governmental decision-making processes and the variation in mobilization of oppressed groups, but this literature (with exceptions in the sociological literature) has largely ignored variation in business preference at the firm level (Fligstein 1990; Laumann and Knoke 1987; Mizruchi 1992).[8] The relationship between business organization at the firm level and corporate preferences for welfare state policies has been totally ignored.[9] But one can extrapolate from other settings three factors that might contribute to preference formation and political mobilization within a firm: the internal policymaking capacities of the company, institutional networks in which the firms participate, and policy legacies of the organization.

HYPOTHESIS 8. *Firms with a Washington, D.C. government affairs office are more likely to develop a supportive position on health reform.*

The internal policy expertise, or capacity for analyzing public policy within the firm, should matter to preference development and political mobilization. State-centered theorists draw conclusions about governmental action from the institutional capacities of states to produce and successfully to implement certain kinds of public policies (Morone and Dunham 1985; Skocpol 1985; Skowronek 1982).[10] In like manner, firms' abilities to grasp complicated policy issues will depend on their own internal political capabilities (Galbraith).

An important institutional development that has heightened understanding of policy issues and increased political participation in many firms is the professionalization of the government affairs function (Post et al. 1983, 135–50). Government affairs departments were originally developed to fight the new social legislation of the 1970s (Baysinger 1984, 248–58). But these units subsequently increased collaboration between business and government (Harris 1989, 261–86; Vogel 1978, 45–78). Companies with these units have more practical views of government (Handler and Mulken 1982, 27).[11] Thus the public affairs functions that first developed to protect the legitimacy of the firm have now become the major locations for cooptation by government.

HYPOTHESIS 9. *Firms belonging to groups that have collectively considered the problems with the health care system are more likely to develop a supportive position on health reform.*

Institutional factor affecting firm political activity consists of the groups and networks that link the firm to the larger community of policy ideas, contributing to information and resource exchange. Interest groups not only represent their member firms' interests but shape their members' preferences (Grimm and Holcomb 1987, 105–18). Mizruchi shows that a company's position in policy networks influences its political behavior (Mizruchi 1992, 13–22).

Groups and networks may be organized along national, regional, or sectorial lines. National groups may make firms more sympathetic to state intervention because they bring business managers into regular contact with government.[12] An economic view of corporate mobilization all too often neglects government leadership in the political organization of business. But government leaders can augment their power against their own political enemies by mobilizing supportive interest groups (C. Martin 1991). Participating in local politics can also sensitize participants to national issues. Historical relations with regional governments may make some firms more sympathetic to state regulations than their counterparts elsewhere (Putnam 1993; Saxenian 1989, 25–70). Sector trade associations also influence firm preference in public policy, because the type of organizing institution at the sectorial level shapes political relations within the industry (Atkinson and Coleman 1989, 47–67; Campbell, Hollingsworth, and Lindberg 1991).

HYPOTHESIS 10. *Companies who have experimented with cost containment within the firm are more likely to develop a supportive position on health reform.*

A final institutional factor—policy legacies—consists of the traditions and past experiences that guide future decision making (Hall 1993; Pierson 1993; Weir 1992). Prior firm strategies offer legacies for coping with political and economic problems (Child 1972, 1–22; Littlejohn 1987, 29; Nelson 1988, 804; Yoffie and Bergenstein 1985, 124–39).[13] As companies exhaust the range of firm-level health care options, they may become disillusioned with market solutions and, through a process of social learning, move on to collective governmental solutions to the problem. (Working against this is the possibility that innovative firms may not favor national reform because they have managed to contain costs, want to protect their competitive advantage in lower health costs, and/or believe that firm-level action is more ideologically appropriate than state action.)

METHODS

The primary empirical goal of the study was to identify the economic and institutional factors that determined a company's support for employer mandates.[14] Some of the data were provided from a series of structured interviews, lasting on average two hours, with high-level managers from a sample of companies. Visits to corporate headquarters of these firms were made in the fall of 1992 and the spring of 1993. In June of 1993, follow-up telephone calls were made to fill in missing data. In all cases, the benefits managers of the firm were interviewed and in many cases, the author also met with the senior vice presidents for human resources and/or with the Washington, DC, government affairs personnel. The evidence gathered directly from company executives was augmented by other data to be described. The sample of companies were a random selection of Fortune 200 manufacturing companies and the American firms on the list of Fortune 500 international service companies (T. Martin and Moran 1992, 208–44; Rogers and Sookdeo 1992, 212–315).[15] Sixty-six percent of a sample of 89 companies participated.[16]

The dependent variable of firm preference was operationalized as the firm's formal endorsement of employer mandates. It consisted of a scale, moving from a formal political position opposing employer mandates to a formal political position supporting employer mandates (Table 1). Thus, firms that were politically inactive on the issue occupied the middle ground.[17] The scale reflected both whether the firm had taken a formal position on employer mandates and the content of that position.

The earlier discussion suggested seven economic hypotheses about the causal determinants of support for health reform. The cost of health care (hypothesis 1) was obtained by asking the firms to calculate their annual contributions to health benefits per worker. (This figure also included company-provided benefits for dependents).[18] The profitability measure (hypothesis 2), taken from 1992 Fortune 500 lists, represented net income as a percentage of sales (T. Martin and Moran 1992, 208–44; Rogers and Sookdeo 1992, 212–315). Size (hypothesis 3) was measured by the total sales of the company, taken from Securities Exchange Commission reports of firm sales for 1991 (Schoch 1993). Unionization (hypothesis 4) was operationalized as the percentage of total employees who belong to a union.[19] Data about the relative capital intensity of the production process (hypothesis 5) consist of a ratio of net sales divided by employees (ibid.).

Another economic argument suggested that regulated and troubled manufacturing sectors were more likely to support national health reform (hypothesis 6). The troubled manufacturing sectors included met-

TABLE 1

Scale Placement and Frequency Distribution of Firm Positions on Employer Mandates

POSITION	VALUE	FREQUENCY	PERCENT
Formal opposition	1.00	1	1.7
Considering opposition	2.00	4	6.8
No formal position but management opposed	3.00	10	16.9
No formal position and ambivalent	4.00	8	13.6
Considering future position but mixed	5.00	3	5.1
No formal position, but a faction of management favored mandates	6.00	8	13.6
Considering support	7.00	8	13.6
Formal support	8.00	16	27.1
Missing data		1	1.7
Total		59	100.0

als, autos, mining, and paper.[20] The regulated sectors included railroads, airlines, utilities, and telecommunications (see also Handler and Mulkern 1982, 24–29). I created a dummy variable to evaluate this effect: firms in regulated and troubled sectors were coded 1, and all others were coded 0. The allocation of a company to a sector was based on the Fortune 500 categorization of firms. The final economic variable, a firm's involvement in international trade (hypothesis 7), was measured using annual reports and respondents' calculations of the percentage of total sales devoted to exports and direct foreign investment. In 14 cases, *firm* data on exports were simply not available; therefore, I substituted a calculation of the *industry's* exports as a percentage of total sales (U.S., Department of Commerce, International Trade Administration 1992).[21]

In addition to the economic variables, the literature suggested several institutional determinants of political action: the firm's internal institutional capacity for politics, external participation in groups, and legacies from previous strategies.

Internal political expertise was operationalized as to whether or not the company had a government affairs office in Washington, DC. This is in keeping with the literature's emphasis on the public affairs function as the major center of political activity within the firm. This dummy variable was coded 1 if the company had a Washington office and 0 if it did not.

It is important to note that the Washington government affairs offices were largely formed in the 1970s and early 1980s, long before national health reform was even a gleam in Clinton's eye. Thus there is temporal priority for the independent variable. In addition, Vogel argues that these offices were largely formed to protect firms' interests against what corporate leaders perceived as the overzealous spirit of new regulation activists in government (Edsall 1984; Vogel 1989). Many of the offices that were originally constructed to oppose government intervention are now pushing firms toward more state support.

To evaluate company participation in groups (trade associations that worked on health reform, regional health care coalitions, and national policy groups), I counted the number of such groups in which a company was a member.[22] I did not include the three major umbrella groups—the National Association of Manufacturers (NAM), the Chamber of Commerce, and the Business Roundtable—in these discussions because almost all the companies belonged to these umbrella associations. In addition, the health policy deliberations within the Business Roundtable and the National Association of Manufacturers have been dominated by health care providers and insurers (C. Martin 1993, 359–93).[23]

One might argue that joining a group is a dependent variable: interest in a solution causes one to join the group. But the participating firms entered into these groups before health reform became a national issue; moreover, the sector and regional groups were established for other purposes. Elsewhere, organizations have been found to enhance and to channel participation. For example, participation in purposive organizations rather dramatically increases participation in campaign activity (Pollock 1982).

Firm-level innovation to contain costs was chosen to evaluate the impact of strategy legacies on support for health reform. Firm-level innovation was best captured by the percentage of employees who belong to managed care networks and health maintenance organizations. The movement into managed care networks began in mid-1980s before companies began seriously to consider national health reform and have been considered the rage in firm-level cost containment.

The first statistical technique used to examine the hypotheses was an ordinary least squares regression method of estimation. Regression analysis was appropriate because of the interval-level quality of the dependent variable (Tabachnik and Fidell 1989, 7–9).[24] Hours of interviewing convinced me that the firms truly were distributed along an ordinal scale with interval qualities. The statistical results were supplemented and complemented by the qualitative interview evidence concerning the dynamics of political participation by the firms. This evidence provided a check and added confidence to the assessments of the importance of the independent variables in the multivariate analysis.

Second, I conducted an F-test to evaluate the relative weights of the economic variables as a group versus the institutional variables as a group. This test was also motivated by a concern about a moderate degree of correlation between some of the variables in each group and a possibility of multicollinearity. I conducted a regression entering the economic variables on the first step and the institutional variables on the second step, and calculated the R-squared change and the associated F-test.

Third, as a mechanism for checking the robustness of our findings, I estimated the model using an ordered probit. The dependent variable was altered somewhat for the probit analysis. Those companies with an explicit formal position favoring mandates were coded 2 (corresponding to the value 800 in Table 1). Those companies that had not taken a position supporting mandates but were not absolutely opposed to them were coded 1. (corresponding to values 4.00–7.00). Those companies in which top management was opposed to mandates were coded 0 (corresponding to values 1.00–3.00).[25] This coding of the dependent variable should reassure anyone concerned about the subtle distinctions in the ordering of Table 1. The companies coded as 2 all took public official positions in support of mandates; those coded as 0 all reported that top management firmly rejected employer mandates.[26]

FINDINGS

At the time of the survey, 28% of the companies supported employer mandates. Over half of the business respondents (54%) personally supported mandates whether their companies had taken a formal position or not, and another 19% felt mixed on the subject. These figures are consistent with other studies. Cantor and his colleagues (1991) found that 80% of the 384 Fortune 500 executives in their study believed that "fundamental changes are needed to make it [health care system] better." Over 32% favored a public health insurance system either now or in the future, and 53% supported employer mandates (pp. 99–101). As one corporate lobbyist put it, "Business from the far right has moved to the center in saying that the federal government needs to be involved." Before turning to political *preference*, it is important to note that the economic structural characteristics were quite relevant to a company's *costs*. Table 2 shows a regression analyzing the health costs per worker. Sales, sector, and unionization were all statistically significant.

But although economic structural characteristics affected costs, these factors were much less important to a supportive position on employer mandates. Turning to the presentation of ordinary least squares regression results in Table 3, equation 1, one sees that none of the economic independent variables was statistically and that only sales and profits had a T-value greater than 1 (or −1). A 1-point increase in sales produced only a .003 point increase in the firm's support for employer mandates; a 1-point decrease in profits produced only a .06 increase in support for mandates.

In reporting the poor performance of the economic variables, one must take into account the size of the sample. Only the largest U.S. firms were included. I chose this sample because the largest firms are more likely to be politically active; thus, it allowed me to understand the dynamics by which political choices are made. But had a more varied population of companies been sampled, size would certainly have been significant. The size of the sample could also have affected the performance of the measure of firm health costs per worker, because all of the companies interviewed provided some health care benefits. In addition, the sample size could have dampened the unionization variable, since larger firms tend to be more unionized. Yet despite the sample size, the firms were enormously varied in many ways. Although all made health care available, some picked up all of the tab while others paid for less than a quarter of their workers' health costs. Some were totally unionized; others had no unionized workers. Profitability varied tremendously, as did the capital-labor ratios of the firms.

Some might be concerned that multicollinearity lay behind the poor performance of the economic variables; yet the variables were only moderately correlated. Percent unionization was correlated with health dollars per worker at .43 and with regulated and troubled sector at .47. Still, to protect against multicollinearity, I conducted a principal component factor analysis with the three highly correlated economic variables: health dollars per worker, percent unionization, and regulated and troubled sectors. These variables seemed theoretically to go together as a type of firm: the regulated, heavily unionized company with generous health benefits such as one finds in the auto or steel sectors. The factor produced accounted for 66% of the variance and had an eigenvalue of 1.977. Yet the new variable produced by this factor (called FT1) had a T-value of only .24 in the regression equation (see Table 3, equation 2).

Given the intercorrelation between variables and the small N, it seemed useful to try to reduce the number of variables in order to give fair play to the competing hypothesis.[27] I pursued two different statistical approaches to reduce the number of variables.[28] The initial approach was to run a series of regressions, beginning with the 10 independent vari-

TABLE 2

Determinants of Firms' Health Costs Per Worker

INDEPENDENT VARIABLES	B	BETA
Sales	−.29 (.11)	−.29*** (−2.54)
Regulated sector	71.04 (30.98)	.31** (2.29)
% union	1.30 (.57)	.31** (2.29)
Constant	309.99 (22.70)	— —

Note: Adjusted R = squared = .34, N = 50. Standard errors and T = statistics are in parentheses.
$^{**}p \leq .05$, two-tailed test.
$^{***}p \leq .01$, two-tailed test.

TABLE 3

Determinants of Firms' Supportive Positions on Employer Mandates

INDEPENDENT VARIABLES	EQUATION 1		EQUATION 2		EQUATION 3	
	B	BETA	B	BETA	B	BETA
Economic						
Sales	.003	.16	.003	.16	.003	.13
	(.003)	(1.21)	(.003)	(1.20)	(.003)	(1.03)
Regulated sector	.15	.03	—	—	.75	.15
	(.78)	(.19)			(.61)	(1.23)
Capital intensity	1.05E-06	.09	1.15E-06	.10	—	—
	(1.42E-06)	(.74)	(1.37E-06)	(.84)		
Profits	−.06	−.13	−.05	−.11	−.08	−.18
	(.06)	(−1.02)	(.05)	(−.96)	(.05)	(−1.63)
Health $$	.001	.06	—	—	—	—
	(.003)	(.39)				
% unionization	−.005	−.06	—	—	—	—
	(.01)	(−.35)				
% exports	−.01	−.08	−.01	−.07	—	—
	(.02)	(−.57)	(.01)	(−.57)		
FT1	—	—	.07	.03	—	—
			(.29)	(.24)		
Institutional						
Washington office	1.85	.41***	1.88	.41***	1.27	.28**
	(.62)	(3.0)	(.59)	(3.16)	(.60)	(2.12)
# groups	.26	.22	.25	.21	.29	.24*
	(.18)	(1.48)	.17	(1.47)	(.16)	(1.81)
% managed care	.01	.17	.01	.17	.01	.11
	(.01)	1.35	(.01)	(1.43)	(.01)	.93
Constant	2.89	—	3.15	—	3.52	—
	(1.16)		(.59)		(.49)	
Adjusted R-squared	.39	—	.42	—	.34	—
Standard error	1.68	—	1.64	—	1.77	—
N	47	—	47	—	56	—

Note: Standard errors and T statistics are in parentheses. FT1 is the factor produced by doing a principal component factor analysis with health dollars per worker, % unionization, and regulated and troubled sectors.
*$p \leq .10$, two-tailed test.
**$p \leq .05$, two-tailed test.
***$p \leq .01$, two-tailed test.

ables and discarding them one at a time until the removal of a variable increased the standard error of the equation and decreased the adjusted r-squared. This approach provided me with a model with the following variables: *sales, profits, Washington office, number of groups,* and *percentages in managed care.* The other approach was to test the economic variables and the institutional variables independently and to select those which were statistically significant from each group for inclusion in the final equation. These regressions produced *sales, regulated and troubled sectors, Washington office,* and *number of groups.* I then ran a regression equation including all of the variables identified in two procedures, including *sales, profits, regulated and troubled sectors, Washington office, number of groups,* and *percent managed care.* In this more parsimonious equation (presented on Table 3) profits approached statistical significance with a T-value of −1.63 and a probability of T at .11; yet none of the other economic variables were close to significance.

An F-test assessing the significance of the change in R-squared across the economic-variable and institutional-variable equations gave some reassurance that the weakness of the economic factors was related to something besides multicollinearity. When the seven economic variables were run in a regression alone with the dependent variable, they explained only .13 of the adjusted r-square.[29] The three institutional variables, entered on the second step of the regression, produced a r-squared change of .26 and an F-change of 1.99; the significance F-change was .08.

The qualitative data strongly reinforce this quantitative finding that economic factors only explain so much of the variance. Negative profitability mattered to many firms: respondents at many of the financially weak companies reported that they had tried everything, were pressed to take dramatic measures, and therefore had turned to government as a last resort. As one supporter of comprehensive reform explained, "There is nothing moral or aesthetic about this—it's strictly hard cash." Yet others explained that low profits prevented them from becoming politically involved. Some companies argued that they wanted employer mandates and comprehensive restructuring of the U.S. health system so that they could better compete in world markets; others said that they had to shift costs to their workers to survive internationally. Thus economic factors seemed to work both for and against the support of mandates.

The data strongly support the institutional view of firm political action. The first major finding was that companies with institutionalized internal policy expertise were more likely to take a position in favor of employer mandates. The presence of a Washington government relations office was by far the most robust variable and was statistically significant at the .01 level, with a T-value of 3.0 in the equation with all of the variables together. A 1-point increase in the *Washington government affairs office* independent variable produced a 1.8-point increase in dependent variable (see Table 3, equation 1).

The second major finding was that companies belonging to groups that studied the health issue were more likely to support employer mandates. This variable was not quite significant in the ordinary least squares regression with all of the variables, although it had a T-value of 1.5 (see table 3, equation 1). The *group membership* variable became close to statistically significant in the more parsimonious equation in Table 3, with a T-value of 1.8 and a probability of T at .08. A 1-point increase in group membership produced a .29-point increase in the dependent variable. The somewhat high correlation between the three institutional variables partly interfered with the performance of this variable: group membership was correlated with *Washington government affairs offices* at .38 and with *percent in managed care* at .32.

But the ordered probit estimation of the model with its revised dependent variable found the groups variable to be statistically significant with a *p*-value of .01. The revised dependent variable rather than the different method of statistical analysis was responsible for this difference in statistical significance.[30] Apart from the slightly better performance of the group membership variable, this change in measurement of the dependent variable produced findings virtually identical to those produced with the less truncated dependent variable (see the ordered probit results in Table 4).

The ordered probit model can be used to illustrate the probability of a response falling into each category of the dependent variable; one evaluates the effect of an independent variable by examining the change in probability of the response falling into each category (see Greene 1993, 672–76).[31] In other words, one must imagine what would happen to the dependent variable with an incremental change in the dependent variable.

Table 5 illustrates the effect of changes in sales, Washington government affairs offices and of groups on the probability of supporting employer mandates. Taking the data as they are, the model predicts 23% opposing mandates, 43% in between, and 34% supporting mandates. With an incremental increase of $100 million in sales (very close to the standard deviation), the probability of supporting employer mandates increases to only 38%, and the probability of opposing mandates drops to only 20%. With an incremental increase in membership of one group, the probability of supporting employer mandates increases to 42%, and the probability of opposing mandates drops to 17%. If all of the firms are given Washington government affairs offices, the probability of supporting employer mandates is 41%. With none of the firms having Washington government affairs offices, the probability of supporting mandates is only 13%. The Washington government affairs office variable is statistically significant with a *p*-value of .006. Thus the ordered probit gives one a sense of the strength of the effects of the independent variables.

TABLE 4

Determinants of Firms' Supportive Positions on Employer Mandates

INDEPENDENT VARIABLES	COEF. (ASYMPTOTIC S.E.)	Z (P-VALUE)
Economic		
Sales	.002 (.002)	.93 (.35)
Regulated sector	−.09 (.58)	−.15 (.88)
Capital intensity	1.09E-06 (1.07E-06)	1.02 (.31)
Profits	−.01 (.04)	−.35 (.73)
Health $$	.003 (.003)	1.30 (.19)
% unionization	.00 (.01)	.02 (.98)
% exports	−.01 (.01)	−.11 (.29)
Institutional		
Washington office	1.33 (.48)	2.77** (.01)
# groups	.36 (.15)	2.42** (.015)
% managed care	.005 (.01)	.74 (.46)
ˆ cut 1[a]	2.10 (1.03)	—
ˆ cut 2[a]	4.03 (1.12)	—

Note: Pseudo R-squared = .34, N = 47.

[a] The lambda cuts refer to the divisions between response categories of the dependent variable. Thus cut 1 separates those opposing mandates from those in between; cut 2 separates those in between from those supporting mandates.

The qualitative data offered repeated testimonials to the importance of groups for preference develop-

TABLE 5

The Effect of Increases in Sales, Group Membership and DC Office on Firm Support for Employer Mandates (%)

CATEGORY OF SUPPORT	AS DATA ARE	PLUS $100 IN SALES	JOIN ONE MORE GROUP	ALL WITHOUT DC OFFICE	ALL WITH DC OFFICE
No	23	20	17	42	11
Maybe	43	42	41	45	48
Yes	34	38	42	13	41

Note: Table 4 uses the ordered probit model (see n. 31) to estimate the probability of a firm falling into each category of dependent variable. Because the coefficients are difficult to interpret, I estimated the effect of the individual variables on the probability of a firm falling into each category in the following way. I first calculated the mean of the probabilities for each firm, keeping the individual variables at their original values. Then I changed the variable I was interested in by a specified amount. For example, I increased sales by $100, and recalculated the mean of the probabilities for each firm.

ment. Groups were the crucible in which participants became aware of their policy preferences. Respondents were often confused about health reform before their groups addressed the issue; most were followers rather than leaders. Many business managers came of age in health care through participation in their trade associations. For example, the American Iron and Steel Institute brought large and small companies with varying profitability together in a unified industry position. In the Edison Electric Institute (EEI), a spirit of cooperation dating from the nuclear energy crisis enabled the industry to take a proactive lead in the health policy arena (Campbell, Hollingsworth, and Lindberg 1991). One participant reported that her participation in the EEI task force on health care radically altered her perspective: "On most issues I am a hard-core Republican, but I'm radical on this issue. I generally don't believe in regulation, but regulation should be when the market breaks down, and it has in health care. I know that I sound like a bleeding liberal, but we need to know that each person will be accounted for. Maybe employers will have to pay more, but at least it will be explicit."

Regional associations were quite important to other companies' path to political activism. Area coalitions were formed before health reform became an important issue and were created to try to control local markets. But community activism at the local level, even if originally oriented to market solutions, led to activism at the national level. Information was an especially important benefit of the groups: the case studies provided numerous testimonials to the learning effects of community activism. Members were invited to participate by contacts in the area and joined for a variety of reasons. But once they began to think systematically about the issues, their perspectives expanded greatly. Although the percentage of unionization in the company was not significant to a firm's political action, labor-management committees also seemed to help many business managers to think about the issues in new ways.

National groups also helped companies to become aware of health reform issues. In fact, some respondents felt that the national groups were so eager to participate in the policy debate that these groups committed themselves too quickly to the reform position. These skeptics worried that the desire to participate made it harder for the business perspective to be represented. One industry representative said: "I am also concerned that the Chamber of Commerce, NAM, and APPWP [Assn. of Private Pensions and Welfare Plans] are all trying to develop a strong position and to show the administration that they can be flexible. But it is too early to put stakes in the ground. . . . In the past business groups have been viewed as 'we are opposed to everything'. This was not a bad position to take in the Bush/Reagan years when they [the Republican presidents] wanted to say this to Congress. . . . Now there is a new set of dynamics. People don't want to say 'no', but they may be too eager to say 'yes'."

Thus the story told here portrays a core and periphery in the American business community. Companies plugged into national networks learned from their groups and peers, and collectively moved toward a political solution. Peripheral firms, isolated from others in comparable positions, tended to rely on consultants and insurers for information and advice and as a result had more conservative views about the possible restructuring of the health care system.

The third institutional variable, *percentage in managed care networks*, had a more disappointing outcome and was not statistically significant using either the ordinary least squares or the probit estimation of the model. But there was some case study support that social learning from previous strategies, such as firm-level experimentation with managed care networks, led to support for national health reform. As companies exhausted private market solutions, they moved on to the more radical state intervention. Numerous respondents described their path to systemic reform as one of increasing frustration with firm-level efforts to change provider behavior. One veteran discovered her support for a single-payer system in completing a survey questionnaire to identify the solution to the U.S. health crisis: "I realized at that moment that the only thing that would make a difference was to have a national solution."

The argument has been made that the development of corporate policy capacity makes a difference. But can't one simply say that this actually reflects an underlying causal effect of firm culture? Firm culture may be behind both the tendency to support health reform and the development of political capacity within companies.

Although we could not find a good quantitative measure of firm culture, the qualitative findings suggest that companies may be very different but that culture is not uniform throughout the company. Firms must determine where to put their energies, and institutional departments differ. A benefits manager reported that shortly before the 1992 election, President Bush asked the firm's chief executive officer to support the president's health plan. The CEO sought input from our respondent, who argued against such action because it might complicate the firm's relationship with the White House should Clinton be elected. The benefits manager, a supporter of play or pay, thus pushed a very Republican company to the center on this issue. Culture may also be less important because the left-right distinctions on health care are beginning to change. Thus one respondent explained, "I am very conservative, but health care is out there by itself."

CORPORATE ENGAGEMENT WITH THE CLINTON PLAN

Despite the early corporate interest in systemic health reform, in the winter and spring of 1993 many large employers distanced themselves from the Clinton health plan. Business objections to the Clinton plan did not, however, translate into a rejection of the goal of universal coverage. Most of the firms in this study that originally supported an employer mandate held to this position throughout the legislative cycle, primarily working for this goal through the Association of Private Pension and Welfare Plans, the Corporate Health Care Coalition, and the Chamber of Commerce health care task force.[32] (These were known as the "mandate" business groups.)

But despite general support for mandates, employers had three problems supporting Clinton. First, big business did not like Clinton's *managed competition* proposal. Sold as a mechanism to control costs while preserving the employer-based system, the health alliances in the administration's original proposal (a firm had to have over 5,000 employees to opt out) made many firms fear that they would be pushed into these regional entities. Because health alliances were to extend across entire regions, companies worried that they would lose their considerable purchasing power relative to the public pools and would receive very poor rates from providers. Thus, Richard Smith with the APPWP argued that the major sticking point for business was not mandates but the strict rules surrounding the health alliances (Smith 1994). Companies also considered the original minimum benefits package excessive, worrying that they would no longer have control over their own plans and would be transformed into "check writers." Business managers felt that the administration engaged in a fiscal slight-of-hand by suggesting that health reform could be achieved without any tax increase, and some believed the cost control mechanisms to be excessively weak in the president's plan. Finally, in moving through the legislative process, the bill gave ever larger subsidies to small business and put heavier burdens on big employers.

Second, big business had a hard time organizing for health reform. Business associations were unable to express the preferences of a majority of their members' for health reform, since these groups were crippled by the objections of a vocal minority (usually insurers and pharmaceutical companies.) Advocates of health restructuring struggled within each of the major business organizations to secure endorsement for the reform proposal; yet, none of the umbrella organizations overcame the divisions within its ranks and supported reform (or, in the Chamber's case, remained in favor of support). The inability of business groups to endorse the parts of the reform proposal that the majority favored made both the administration and Congress less willing to take big business inputs seriously. Politicians wanted active help in getting votes in exchange for attention to corporate concerns. Confronted with the weakness of big business, legislators increasingly granted concessions to the much better organized small business community to try to buy off its opposition.

A final factor contributing to limited support by big business was Clinton's conflicting priorities and strategies. Although the president developed a product designed to appeal to business, the pressing claims of the economic package and NAFTA finally delayed health reform and early corporate enthusiasm subsided. Unlike Lyndon Johnson, who used corporatist-style task forces to build consensus, Clinton excluded business in his task force, using the process primarily as a policy development tool. Originally the administration promised to address large employers' major concerns at the proposal development stage, but Congressional allies forced a retreat on early deal making. The administration also used a language of class warfare to try to mobilize populist support; the villains in health reform were drug and insurance companies (Woodward 1994). But its language of good and evil elicited emotional, ideological responses that undercut the ability of business policy experts to portray the issue in technocratic terms. The Clinton's language of class warfare worked against the policy wonks within the firms.

Big business supporters were further hampered by a Republican and small business campaign of obstruction. Believing that the administration could be conquered with the defeat of health reform, Republican legislators dramatically attempted to prevent business from supporting reform by threatening retaliation in future policy areas. When the Chamber of Commerce endorsed an employer mandate and a standardized benefits package, the House Conservative Opportunity Society chastised the Chamber leadership. John Boehner (R-Ohio and chairman of the group) sent letters on congressional letterhead to

Chamber constituents, saying that they should cancel their Chamber membership.

CONCLUSION

Companies across America acknowledged a need for employer mandates and health reform to cure the hemorrhagic health care system. I have analyzed the causes of variation in firm response to the health crisis. Although escalating costs and frustration with market intervention precipitated the willingness to explore government intervention, economic factors alone were insufficient to account for the change. New attitudes about social regulation in health care found a sympathetic ear among firm policy professionals. The institutionalization of their political capacities made companies more open to technical arguments about state solutions. Businessmen were exposed to these ideas in groups and networks. At least some companies went through a process of social learning in which frustrations with prior failures made them open to fundamentally different solutions.

These findings have important implications for the way that we think about business political action, offering very different insights from those suggested by the pluralist, instrumentalist, or structural power theorists. Conventional views of business political mobilization suggest that interests are easily recognized and that solutions are readily apparent. Yet health reform illustrates that preference formation takes place in collective organizations as policy personnel search for new solutions to a social problem.

Conventional research designs often isolate economic structural characteristics to the neglect of institutional factors in accounting for companies' policy preferences. Yet I have suggested that among the largest companies, corporate policy capacity is vital to outcomes. This sample's focus on the largest companies to some extent dampens the influence of size and cost, limiting the generalizations that can be derived from the study. But the in-depth study of the largest firms presented here also provides insight into the way that size mattered. It is the institutional slack often linked to size and the capacity of large firms to develop their political powers that make large companies important political actors.

The primacy of economic concerns to corporate managers is very real. This is not a story of corporate actors engaging in altruistic choice of the public good over their own firms' interests. Rather, it is a story of business managers' struggle to locate their interests in a world of imperfect knowledge and both long- and short-term considerations. Thus the institutional approach adds a context for the interpretation of interests, an explanation for why problems become political issues, and a view of the corporate road to political participation.

First, this approach highlights the importance of technical information to corporate preference formation. Ideas and interests are often portrayed as distinct and somewhat mutually exclusive explanations for policy origins (Heclo 1974, 298, 304–5; Weatherford 1986, 36–59). The split between ideas and interests often mirrors the split between state and society, with state actors guided by ideas and societal figures acting according to interests (Weir and Skocpol 1985, 107–63). Yet this study suggests that ideas shape firm positions.

Second, this view suggests that resources for political mobilization among business are both organizational and monetary. Firm preference and political action depend on the organization of professional expertise and the networks that expose employers to new ideas about social issues. We must put these insights into perspective: other policy spheres may have different politics (Lowi 1964). Interests are undoubtedly more fixed in areas closest to the production process; social initiatives may give business participants somewhat more freedom. Institutional context also matters the most during periods of ideological uncertainty: when old paradigms lose their salience, the interpretation of material interests becomes less obvious and the social construction of interests becomes more significant (Salant 1989, 37).

Third, businessmen are captured by fads just like the general public. Innovations in cost containment have gone through something of a reform cycle phenomenon, as market and regulatory solutions gain favor and fade. Morone argues that support for republican values occasionally guides Americans toward a different kind of politics and policy. People become enthusiastic about reforming the political process, then politics as usual set in and they become disillusioned with their innovations (Morone 1990). Business managers also exhibit cycles of enthusiasm for reform, supporting state activism in some periods but not others.

What do these findings imply for public policy? The constructive aspect of preference formation suggests greater business support for social welfare innovations than otherwise supposed. Most of the welfare state literature assumes that social policy initiatives prosper when business is relatively weak, especially vis-à-vis labor. Although often correct, this assumption misses the conditions under which business plays a more positive role in the process. With the decline of labor, the structural change in the organization of work, and the growth of issue-oriented politics all over the world, future welfare initiatives may depend more than ever on cross-clamp coalitions (Lasch 1985, 215–39; Longstreth 1988, 413–32; Swenson 1991, 513–44). The case of national health reform suggests that these cross-class coalitions are possible.

Yet formidable constraints work against business support for social initiatives as well. The two kinds of factors leading to corporate political participation—economic motivation and institutional facilitation—are in something of a dialectic tension with one another. This discontinuity makes the business contribution to public policy problematic. Corporate policy experts wear two hats: they are *both* bearers of the economic interest of the firm and health care policy professionals with enormous accumulated knowledge and expertise.

As expert participants in the policy debate, firm policy professionals search for broader collective solutions to the problem; but at the same time they represent the firm's interests. This Janus-faced perspective helps government policymakers when business professionals take their technocratic views back to the firm, yet businessmen's concerns also limit the reform options. The overlapping and contrary nature of these two motivating and shaping influences is what makes the role of business in national health reform so confusing. Contradictions existing at the level of preference between the material and the ideological are mirrored at the level of policymaking.

APPENDIX

TABLE A-1

Descriptive Statistics

VARIABLES	MEANS	CORRELATIONS WITH INTEREST
Economic		
Sales ($billion)	9.38	.35*
Total health costs ($billion)	1.56	.35*
Costs per worker ($)	3,460 (annual)	.02
Profits	3.41	−.17
% unionized	29	.26
Capital intensity	232553	.17
Regulated sector	.26–26% in sectors	.35*
Exports as % of total sales	16	−.09
Institutional		
Washington Office	.66–66% have	.46**
Number of groups	2.3	.49**
% of workers in managed care	33	.37*

*$p \leq .05$.
**$p \leq .01$.

TABLE A-2

Correlation Matrix: Economic Variable

VARIABLES	INTEREST	HEALTH $	PROFITS	SALES	% UNION	CAPITAL INT.	REGULATED SEC.	% EXPORT
Interest	1.0000	.0160	−.1668	.3544*	.2645	.1751	.3535*	−.0954
Health $$	.0160	1.0000	.0316	−.2982	.4289**	−.0928	.4662**	−.2011
Profits	−.1668	.0316	1.0000	−.1307	−.2530	.1459	.0400	−.0157
Sales	.3544*	−.2982	−.1307	1.0000	.0106	.1076	.0493	.0006
% Unionized	.2645	.4289**	−.2530	.0106	1.0000	−.1483	.4860**	−.3330*
Capital Intens.	.1751	−.0928	.1459	.1076	−.1483	1.0000	−.0376	.0920
Regulated sect.	.3535*	.4662**	.0400	.0493	.4860**	−.0376	1.0000	−.4042*
% Export	−.0954	−.2011	−.0157	.0006	−.3330*	.0920	−.4042*	1.0000

*$p \leq .01$, two-tailed test.
**$p \leq .001$, two-tailed test.

TABLE A-3

Correlation Matrix: Institutional Variable

VARIABLES	WASHINGTON OFFICE	GROUPS	% MANAGED CARE
Interest	.4645**	.4881**	.3674*
DC office	1.0000	.3768*	.3841*
# groups	.3768*	1.0000	.3176*
% managed care	.3841*	.3176*	1.0000

*$p \leq .01$, two-tailed test.
**$p \leq .001$, two-tailed test.

Notes

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1. Instrumentalists locate corporate influence in the predominance of the capitalist class in positions of power in both the public and private spheres (Miliband 1969). Structural power theorists believe that the state's structural dependence on private investment gives business a veto over public policy through the threat of disinvestment. Thus, business wields an enormous amount of control over state policymaking, regardless of whether it actually takes action.

2. Health care tied to employment prevents people from moving to better jobs, because many plans have *preexisting condition* clauses.

3. For example, a firm with sufficiently concentrated interests or a sufficiently large part of the collective good might be willing to bear all of the costs.

4. The biggest companies are subject to cost shifting from smaller firms in their provision of health benefits to spouses of workers.

5. We gathered data on two other workforce characteris-

tics that subsequently had no explanatory value: the age of the workforce and the number of retirees to active workers.

6. I also gathered data on the percentage of a firm's total sales devoted to the health industry, but this was not significant.

7. I include under the rubric of institutionalists state-centered theorists such as Skocpol and Skowronek, social movement theorists such as Tarrow and McAdam, institutional sociologists such as Mizruchi, and rational choice new institutionalists such as Moe.

8. Sociological network analysis has concentrated on networks, unfortunately neglecting other institutional causal factors.

9. Most assume that business hinders welfare state development (Korpi 1980, 296–315; Stephens 1979; see also Berger 1981).

10. One difference between state capacity and firm capacity is that state capacity looks at *both* the ability to form policy and to execute these policies. My use of firm capacity focuses on preference formation rather than on the execution of policies.

11. Chief executive officers blame the pragmatic focus of their political action committees on the undue influence of the Washington representatives.

12. Groups may take different positions, but the mere participation in collective forums make businessmen think in more pragmatic terms.

13. As John Stephens points out, prior strategies are also shaped by economic interests.

14. It is important to note that firms did not develop positions on this issue unless they wanted to become involved; therefore, those companies in the top category were truly politically active on health reform.

15. The sample was chosen from a list sorted by size and sector; every third firm was selected. Data for the service sector firms were taken from the comparable *Fortune* figures for the international 500 largest service corporations.

16. I anticipated that there would be some selection bias in that active companies would be more willing to participate than inactive ones. While this was true to some extent, this tendency was overcome by an opposing tendency for the inactive firms to welcome me as a source of additional information about the health care debate. Also, some known activists in health care refused to see me because they were experiencing severe fiscal constraints and did wish to share information about their health costs or could not afford the resources to participate.

17. If there was no formal position, the position of the highest-ranking respondent at the firm was used as a proxy for content. The benefit manager's view in general seemed to be an accurate reflection of the general views of top management except in eight cases, where the company was quite divided. These cases were presented in the answer "no formal position but respondent favored mandates." The fact that the two measures were correlated at nearly .5 supports the use of a scale.

18. Based on self-reported and seemingly quite reliable data.

19. Based on company-supplied data.

20. These were drawn from a list of the "sick six," which also includes two regulated sectors.

21. This blending of data is unsatisfactory, but the reputed importance of trade concerns motivated the fullest investigation possible.

22. National groups include Washington Business Group on Health, APPWP, ERISA Industry Committee, Employee Benefit Research Institute, and the National Leadership Coalition. Firms received one point for sitting on the task force of a group (but could receive only one point).

23. The big umbrella business groups tend to sink to lowest-common-denominator politics.

24. The interviews with corporate managers suggested a rather smooth progression from a strong negative position opposing mandates, through weak rejection of or support for mandates with no formal position, to a strong positive position in favor of mandates. The distance between each point on the scale seemed approximately equal; thus the data take on many attributes of continuous data. The parameters of the quantitative regression equations resulting from the statistical calculations were consistent with the qualitative data of the interviews.

25. Those coded as opposing mandates had not, with one exception, taken a formal public position; however, the company respondents reported that top management within the firm was uniformly opposed to the measure. The tendency for only those supporting mandates to take a formal position reflected the nature of the issue. Since mandates promised a general rather than a narrowly targeted impact, firms were less likely to develop the kinds of defensive positions protecting themselves that one finds, for example, in very specific environmental regulations.

26. The revised dependent variable had a distribution of 16 supporting mandates, 27 neither supporting nor opposing, and 14 opposing.

27. The parsimonious equation also allowed us to increase the n of the equation, since PCEXPORT and HEALTH$WOR both contained missing data.

28. This two-route approach resembles that taken by Skocpol et al. 1993. The authors limited themselves in their first route to only those variables that were statistically significant after discarding variables from the equation one at a time (pp. 686–701). Had we limited ourselves to statistically significant variables, we would have been left with only District of Columbia office and number of groups. Therefore, we decided to include all variables in this first round that did not increase the standard error when added.

29. The highest adjusted r-squared using only economic variables was .20, produced when sales and regulated sector were run alone. The institutional variables run alone produced an adjusted r-squared of .35.

30. The two key institutional effects held for an OLS estimation of the model of the truncated dependent variable. The OLS regression using the three-point dependent variable produced virtually the same results as the ordered probit estimation of the model. I felt confident using the OLS method because the three-point scale approximated a normal distribution with 16 supporting, 27 in between, and 15 opposing. In this equation, the District of Columbia office variable was statistically significant at the .01 level with a T-value of 3.0. The truncated dependent variable had exactly the same T-value as the 8-point dependent variable and was comparable to the ordered probit Z-level of 2.8. The group membership variable was also statistically significant at the .05 level with a T-value of 2.2 (comparable to the ordered probit Z-level of 2.4). None of the economic variables had a significance of T lower than .27. The adjusted R-squared of the OLS regression run with all of the variables and the truncated dependent variable was .40 (again comparable to the adjusted R-squared of .39 using the 8-point dependent variable) and the standard error of the equation was .59.

31. The ordered probit model assumes a latent-variable structure of the form $y^* = XB + e$. We observe

$$y = 0 \quad \text{if } e < XB, \quad y = 1 \quad \text{if } XB < e < XB + \hat{}\,,$$

$$y = 2 \quad \text{if } XB + \hat{}\, < e,$$

where $\hat{}\, > 0$, and

$$P_0 = F(XB), \quad P_1 = F(XB + \hat{}\,) - F(XB), \quad P_2 = 1 - F(XB + \delta).$$

$\hat{}$ signifies lambda. The lambda cuts specified on Table 3 refer to the divisions between response categories of the dependent variable. Thus cut 1 separates those opposing mandates from those in between; cut 2 separates those in between from those supporting mandates.

32. In the spring of 1995, I contacted all of the companies in my study that originally either supported employer mandates or were thinking about taking a position (i.e., were coded 7 or 8 on the scale). (I did not attempt to contact all of the firms because it seemed unlikely, given the climate surrounding the health debate, that those who originally opposed employer mandates would come to support them, and I did not want to lose cases in the event that less interested firms failed to return my telephone calls.) Two of those ranked as 8 contin-

ued to support universal coverage but moved away from employer mandates, and one large company was silenced on the issue due to objections from small business customers. All but 3 of the 16 firms originally coded as "8" continued to support employer mandates. The three others remained committed to universal coverage but were silenced by consumer pressures or became pessimistic that a reasonable mandate could ever be created by the Clinton administration. Five of the 8 initially coded "7" (who were seriously considering taking a supportive position, but had not yet done so) moved into the "8" category of having a formal position supporting mandates. One company originally coded "7" initially worked very closely with the administration but ultimately turned away from mandates, frustrated by the administration's version.

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DOMESTIC INSTITUTIONS AND INTERNATIONAL BARGAINING: THE ROLE OF AGENT VETO IN TWO-LEVEL GAMES

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Putnam's (1988) conjecture that negotiators can benefit from their domestic constraints implies that they may want to impose domestic constraints on themselves by granting veto power to an agent. I show that a negotiator's decision to employ an agent as veto player depends on the kinds of information available to the foreign country and the alignment of preferences between the negotiator and the agent. When the foreign country has incomplete information about the negotiator's preferences and the negotiator has preferences too divergent from those of the agent, the negotiator will not give veto power to the agent. However, this applies only to an agent with extreme preferences, and a surprisingly large number of agent types will receive veto power. The attractiveness of the agent veto to the negotiator is in part due to its informational effect. By granting veto power to an agent, the negotiator can transmit more information to the foreign country and capture informational gains that would be lost in the absence of the agent veto.

According to Putnam (1988), two-level games characterize situations in which a negotiator finds herself simultaneously engaging in both domestic and international bargaining. Domestic bargaining is necessary because an international agreement is usually subject to ratification. This requirement constitutes a negotiator's domestic constraint, limiting the range of proposals that the negotiator can accept. Putnam shows that a negotiator can actually strike a better bargain when she is subject to such a domestic constraint; the negotiator can use the hard-line position of her domestic constituent to extract more concessions from a foreign country.[1]

I shall take Putnam's argument one step further and ask whether a negotiator will actually prefer to impose a domestic constraint on herself by granting veto power to her domestic constituent. If a domestic constraint is as beneficial as Putnam suggests, the negotiator will certainly want to have it as a bargaining tool. More broadly, I shall use Putnam's model of two-level games to address the issue of institutional design in international negotiation theory. If ratification procedures affect international bargaining outcome with significant distributive consequences among domestic actors, it is unreasonable to think that ratification procedures are exogenously given (Evans 1993, 415; Pahre 1994). Those domestic actors with stakes in international bargaining outcome have an incentive to choose a domestic bargaining institution favorable to their interests. Although Putnam suggests that an international negotiator has such an incentive, the conditions under which she will actually give veto power to another domestic actor have not been derived.[2]

In the model presented, there are two domestic actors, a principal and an agent.[3] The principal as the negotiator is assumed to have the power to design a ratification procedure (i.e., whether to give veto power to an agent). Unlike Putnam, I do not restrict the principal-agent relationship in my model to the executive-legislative conflict. The agent in the model does not have to be a legislature; it can be an interest group, an executive branch agency, or a congressional committee that can exercise de facto veto power over an international agreement. Likewise, the negotiator in the model does not have to be a chief of government. In the case of trade policymaking in the United States, it is more reasonable to think of Congress as the principal and the president as her agent because Congress with its constitutional authority can design trade policy institutions and shape trade negotiations.

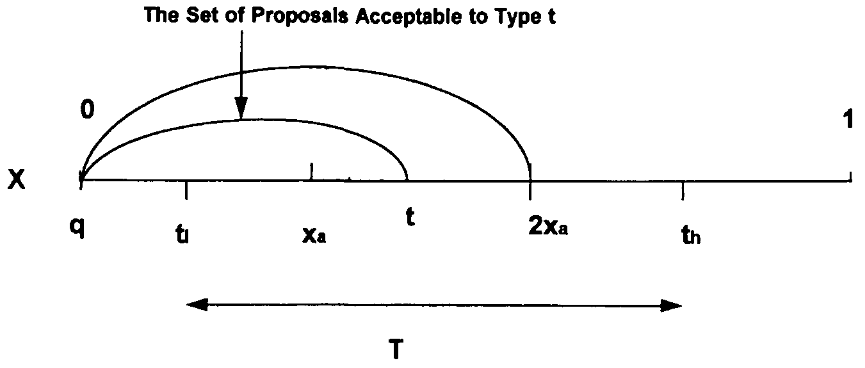
The agent, if given veto power, can approve or reject an agreement that has been negotiated with a foreign country. Since the only source of domestic player's bargaining power in the model is their veto power, the agent plays no role in the game without the agent veto. This model best captures Putnam's logic of two-level games in which international bargaining is dominated by the negotiator's search for ratifiable agreements.

If the agent is given veto power, the foreign country must make a proposal acceptable not only to the principal but also to her agent. As Putnam suggests, this new constraint may benefit the principal by forcing the foreign country to make more concessions. In fact, I will show that when the foreign country has complete information on the principal's preferences, it is always better for the principal to grant veto power to the agent.

If the foreign country has incomplete information about the principal's preferences, the principal's incentive to use the agent veto weakens; it now depends on the closeness of the preferences between the principal and the agent. However, even in this case the principal's incentive to use the agent veto remains strong in the sense that the principal avoids the agent veto only if her agent has interests very divergent from the principal's own. The principal's preference for the agent veto is in large part due to its

FIGURE 1

Configuration of Preferences



informational effect, an element not considered by Schelling and Putnam. That is, the use of the agent veto affects the way information is transmitted during the negotiation.

Since the foreign country must seek the agreement of the agent with veto power, it may propose an outcome too far from the principal's ideal outcome. In that case, some types of the principal will reveal their true type to induce the foreign country to make a proposal closer to their ideal outcome. In other words, if an agent with veto power has an extreme position against the foreign country, moderate types of principal will reveal their true preferences instead of trying to bluff the foreign country. If they bluff, they know that the foreign country will make a proposal too close to an agent's position, so they do not have an incentive to bluff. Without the agent veto, however, the principal cannot credibly send information to the foreign country because the principal cannot credibly commit herself to not bluffing. Thus, when the agent has veto power, the principal is able to transmit information to the foreign country and capture informational gains that would be lost in the absence of the agent veto.

The agent's veto power, however, imposes costs on the principal. The principal suffers distributive losses when the agent's veto power forces the foreign country to make a proposal close to the agent's ideal outcome. Therefore, in choosing to grant veto power to the agent, the principal faces a trade-off between informational gains and distributive losses. How the principal resolves this trade-off depends on the alignment of preferences between herself and the agent. If the ideal outcomes of the principal and the agent are relatively close, the principal will use the agent veto; otherwise, the agent does not receive veto authority.

THE MODEL

A principal (P) and an agent (A) representing one country bargain with a foreign country (F) to determine a policy outcome $x \in X$ where X is assumed to be the unit interval. The status quo or disagreement outcome q in X is 0. The players' preferences in X are assumed to be single-peaked with an ideal point. To simplify the analysis, I further assume that their utility functions are quadratic and symmetric around their ideal point and are represented as

$$u_p = -(x - x_p)^2,$$

$$u_a = -(x - x_a)^2,$$

$$u_f = -(x - x_f)^2,$$

where x_p, x_a and x_f are P, A, and F's ideal points in X, respectively. I assume $x_f = 1$, indicating that F is least satisfied with q. I also assume that both P and A prefer some deal to the status quo; that is, $x_p, x_a > 0$. Otherwise, A can obstruct the negotiation by rejecting any agreement greater than q, and P has no incentive to employ such an agent. P and A's preferences over policies with respect to q are illustrated in Figure 1.

The information structure of the model is as follows. F has incomplete information on P's preferences, that is, x_p.[4] Negotiators do not have precise information on the preferences of their foreign counterparts and are placed in a position of "figuring out" what they want. But F is assumed to have complete information on A's preferences. Unlike the principal who, as the negotiator, has to represent a diverse group of domestic interests, the agent is assumed to represent a much narrower domestic interest and thus to have relatively well known preferences; that is, the agent has established its reputation as a hawk, a dove, or a moderate.

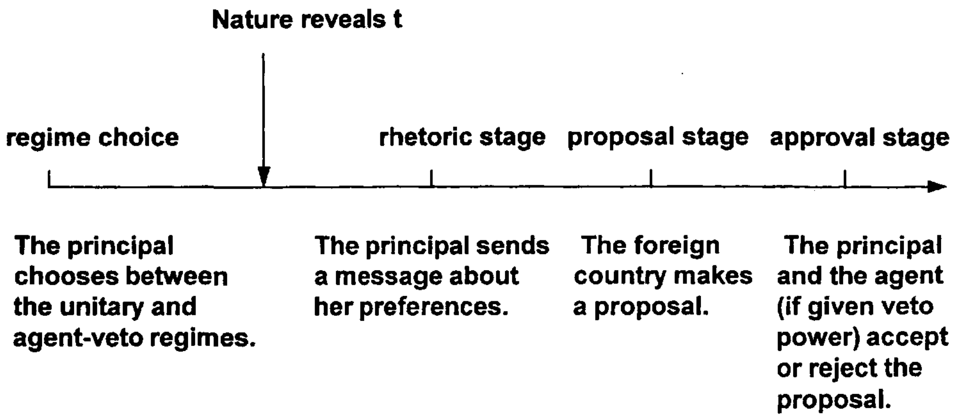
In order to facilitate the exposition, P's type, t, will be assumed to represent not its ideal point x_p but the outcome $2x_p$ that gives the same utility to P as the status quo, q. The set of P's types is $T = [t_l, t_h]$, and this means that P's ideal points are distributed over

$$\left[\frac{t_l}{2}, \frac{t_h}{2}\right].$$

This parameterization identifies the set of points

FIGURE 2

The Sequence of Moves


Nature reveals t
regime choice
rhetoric stage
proposal stage
approval stage
The principal chooses between the unitary and agent-veto regimes.
The principal sends a message about her preferences.
The foreign country makes a proposal.
The principal and the agent (if given veto power) accept or reject the proposal.

that P prefers to q. For type t, this set is $[0, t]$. In order to preclude situations in which P favors F's ideal point over q, we assume that $t_h < 1$. Similarly, the set of points that A prefers to q is $[0, 2x_a]$, and $2x_a < 1$. It is highly unlikely that domestic actors favor a foreign country's ideal outcome over the status quo.

P's types are drawn from a density function $f(\cdot)$ over the interval $[t_l, t_h]$. This density function $f(\cdot)$ is common knowledge but, as noted, the realization of t is private information to P.

The game follows a sequence of moves as illustrated in Figure 2. At the initial regime choice stage, P chooses the agent-veto or unitary regime, depending on whether P grants veto power to A. After P chooses a regime, P's type is revealed to P.[5] P is assumed to choose a bargaining regime before it knows its future preferences with certainty because a bargaining regime or institution, once created, is difficult to change, especially in international negotiations. For example, although P may anticipate that her preferences over trade policy shift as a result of change in economic conditions, it will be too costly to revise trade laws or institutions simply as a function of economic conditions. P, however, has expectations about her future type t which coincides with those of F; that is, $t \sim U[t_l, t_h]$.

Once a regime is chosen and private information is revealed, P and F play a three-stage bargaining game similar to that of Matthews (1989). At the rhetoric stage, P sends a message about t to F. F responds to it by proposing a policy x at the proposal stage. At the approval stage P and A (if given veto power) accept or reject x. If the proposal is rejected by either player, the final outcome is the status quo, q.

For each type $t \in T$ of P, P's pure strategy signal rule is $m:T \to S$, where $s = m(t)$ is the message sent by P of type t and $S \equiv T$ is the set of messages.[6] F's strategy set at the proposal stage is the set of all pure strategy proposal rules mapping messages into proposals: $\pi:S \to X$.

P's acceptance rule at the approval stage is a function $\alpha_p(x, t):X \to \{0, 1\}$, which takes a value 1 if a P of type t accepts proposal x and 0, otherwise. A's acceptance rule $\alpha_a(x):X \to \{0, 1\}$ is similarly defined.

There are many Nash equilibria in this game. I use the concept of sequential equilibrium to focus on a reasonable equilibrium. A sequential equilibrium requires four things:

1. At the approval stages, both P and A (if A is given veto power) never reject a proposal that they (weakly) prefer to the status quo; otherwise, their strategies are not subgame perfect. That is, they cannot commit to act in a manner not in their interests at the approval stage.
2. P sends a message to induce its most preferred proposal, given F's equilibrium proposal strategy.
3. F's updated beliefs are consistent with P's optimal signaling strategy.
4. F's proposal is sequentially rational; that is it maximizes F's expected utility, given F's updated beliefs and P's (and A's) optimal veto strategies.[7]

When there are multiple sequential equilibria for a given set of parameter values, the Pareto criterion will be used to select the most reasonable equilibrium; that is, an equilibrium is Pareto-optimal if no other equilibrium gives higher expected utility to either P or F without lowering the expected utility of the other. Therefore, when a nonpooling equilibrium exists, it must be shown that the players prefer that equilibrium to, say, a pooling equilibrium.

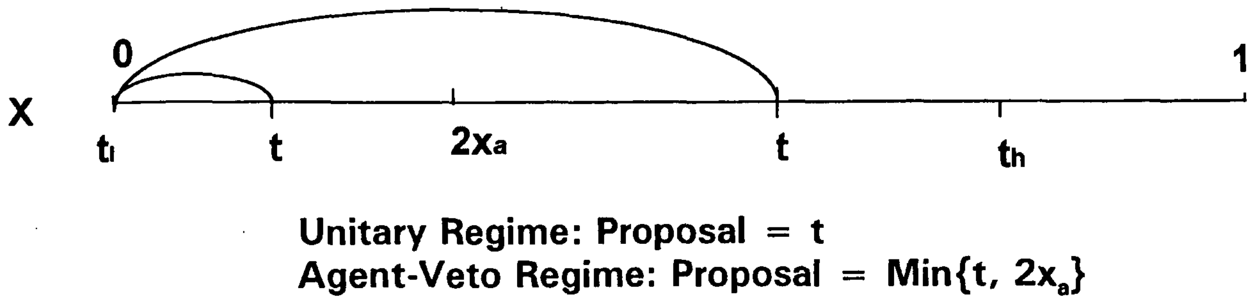
THE COMPLETE INFORMATION CASE

I shall now characterize the complete information equilibrium as a benchmark case. The complete information case also corresponds to Putnam's model, so I expect to find similar results.

When A does not have veto power and F has complete information, F will always offer $x = t$. Without A as a participant, F knows that P will accept any proposal in $[0, t]$. Thus, F will proposes t because

FIGURE 3

Equilibria under Complete Information



0
1
X
t_l
t
$2x_a$
t
t_h
Unitary Regime: Proposal = t
Agent-Veto Regime: Proposal = Min{t, $2x_a$}

it is the best outcome for F among the proposals in $[0, t]$.

When A has veto power, F must offer a proposal that is acceptable both to A and P. In this case, F knows that in order to obtain P and A's approval, x must lie in $[0, 2x_a]$, as well as $[0, t]$. Given this, it is straightforward to show that F will offer $x = \min\{t, 2x_a\}$. Note that when $t < 2x_a$, A's veto power does not reduce the size of the domestic win-set which remains at $[0, t]$. Thus Putnam's assumption that P can freely manipulate the size of the domestic win-set does not hold when A has preferences closer to F than P or A is more "dovish" than P. The equilibrium proposals in the complete information case are shown in Figure 3.

When $x_a < t/2$ (i.e., A is located farther from F than P), P ex post prefers the agent-veto regime because it prefers the agent-veto outcome $2x_a$ to the unitary outcome t. When $x_a \geq t/2$, however, P is indifferent between the agent-veto and unitary regimes because it gets t in either case; that is, when A is more opposed to F than P is, P can use A's veto power to extract more concessions from F. This is one illustration of Putnam's logic of two-level games.

Since P is never worse-off ex post with the agent veto, it is clear that P will also ex ante prefer the agent-veto regime to the unitary regime. Before showing this formally, let me make further simplification of the model by assuming that $t_l = 0$ and that t is uniformly distributed over its domain. These assumptions will also simplify the presentation of equilibria in later analyses. Given these assumptions, P's expected utility under the unitary regime can be represented as

$$Q = -\int_0^{t_h} \left(t - \frac{t}{2}\right)^2 \frac{1}{t_h}\, dt,$$

since the bargaining outcome is always t. In contrast, P's expected utility G under the agent-veto regime is

$$G = -\int_0^{2x_a} \left(t - \frac{t}{2}\right)^2 \frac{1}{t_h}\, dt - \int_{2x_a}^{t_h} \left(2x_a - \frac{t}{2}\right)^2 \frac{1}{t_h}\, dt,$$

since the bargaining outcome is t in the interval $[0, 2x_a]$ and $2x_a$ in the interval $[2x_a, t_h]$. It is straightforward to show that $G \geq Q$ for all values of x_a as shown in Figure 4.

P prefers to give veto power to A, regardless of A's preferences, because P always gets t under the unitary regime, which is no better than q. Under the agent-veto regime, the bargaining outcome is $2x_a$ which is strictly better than q for some types of P. The other types of P reject $2x_a$ in favor of q, but they are not worse off, because they get only t under the unitary regime.[8]

The result that P will always prefer the agent-veto regime under complete information may be interesting but is not empirically compelling.[9] It is not true that principals always give veto power to their agent, regardless of the latter's preferences. For instance, Congress has been trying to take away the veto power of the president in Section 301 trade negotiations.[10] Moreover, the president does not always submit an international agreement for ratification (Pahre 1994). In parliamentary systems, the prime minister does not have to submit an agreement for ratification at all (Milner 1993). Since the predictions of the complete information case do not seem to be supported empirically, I now turn to the incomplete information case in which P's preferences are private information.

THE INCOMPLETE INFORMATION CASE

Under the assumption of complete information, P always favors the agent veto because it gets only t if it does not use the agent veto. When F has incomplete information on t, however, it can no longer offer t and have it accepted by P under the unitary regime. Thus the unitary regime under incomplete information must be better for P than under complete information. In the following analysis, I will show that when F does not have complete information on t, P

can become actually better off under the unitary regime than under the agent-veto regime.

The Unitary Regime

When F has incomplete information about t and P does not grant veto power to A, only a pooling equilibrium exists. That is, P is unable to transmit any information to F, where "information" means that F revises its beliefs about P's types based on P's message.

To see this, suppose that there are two types of P, m and n, such that $n > m$. Note that type n (m) is indifferent between the status quo and proposal n (m), which is what F offers when it thinks p is type n (m). I also assume that type n's preferences are such that it prefers proposal m to proposal n. Since there are only two types of P, information is transmitted only if P reveals its true type in equilibrium. If it does, F will propose n when P's type is n and m when P's type is m, because F knows in equilibrium that P is revealing its true type. However, given this proposal strategy by F, truth telling cannot be P's equilibrium strategy because type n, which prefers proposal m to proposal n, will mimic type m (i.e., sends a message saying that it is of type m, not type n). Thus the truth-telling equilibrium does not exist, so no information can be transmitted under the unitary regime.

From F's perspective, type n in this example is more moderate than type m. Unfortunately for F, the moderate type n does not want to reveal its type because if it does, F will offer it a proposal that is no better than q. As a result, the moderate type tries to bluff the foreign country to gain more concessions (i.e., mimics the hard-liner [type m]), and P does not transmit any information to F. This result based on the two-type case can be generalized to the continuous type case. The following proposition, the proof of which is provided in the Appendix, formally states this result:

PROPOSITION 1 (THE UNITARY EQUILIBRIUM). *When* A *does not have veto power, no information is transmitted (i.e., only a pooling equilibrium exists) so that* F *must make a proposal based on its prior beliefs.*

The equilibrium proposal x_1 by F (see the Appendix) can be derived as

$$x_1 = \frac{1}{3}\left(2 + t_h - \sqrt{t_h^2 - 2t_h + 4}\right). \qquad \textbf{(1)}$$

In this equilibrium, P rejects F's proposal with positive probability. Since the type $t \in [0, x_1)$ rejects proposal x_1, the probability of ratification is

$$\frac{t_h - x_1}{t_h}.$$

Figure 5 graphically illustrates the unitary equilibrium.

P's expected utility N in the unitary equilibrium is

$$N \equiv -\int_0^{x_1}\left(q - \frac{t}{2}\right)^2 \frac{1}{t_h}\,dt - \int_{x_1}^{t_h}\left(x_1 - \frac{t}{2}\right)^2 \frac{1}{t_h}\,dt.$$

When $t \leq x_1$, P rejects F's equilibrium proposal x_1, so q is the bargaining outcome. When $t \geq x_1$, however, the bargaining outcome is x_1. Note that N is constant in x_a. Since A does not participate in the negotiation, P's ex ante utility is independent of A's preferences.

The Agent-Veto Regime

When A has veto power, P can credibly send information to F. To see this, note that when A has veto power, F never makes a proposal greater than $2x_a$, regardless of its beliefs about t, because such a proposal will be rejected by A with certainty. Therefore, F's proposal must lie in $[0, 2x_a]$.[11] In particular, F will propose $2x_a$ if it believes that $t \in [2x_a, t_h]$. When t is greater than $2x_a$, P knows that if it tells the truth

FIGURE 4

Unitary Versus Agent-Veto Regimes with Complete Information

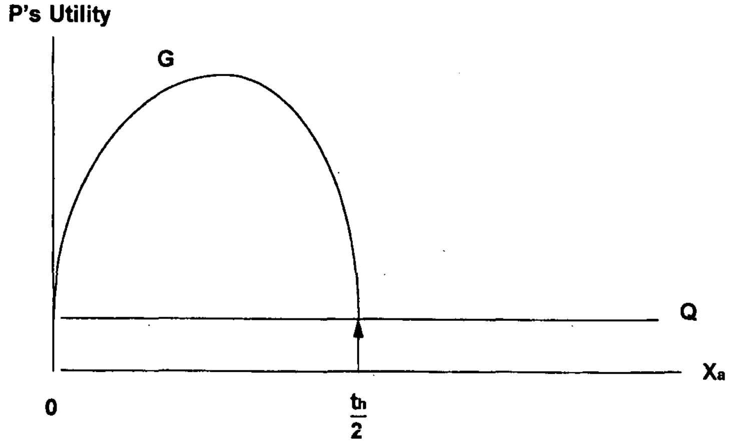
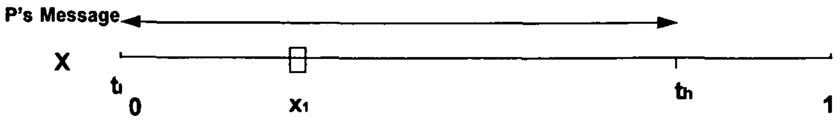
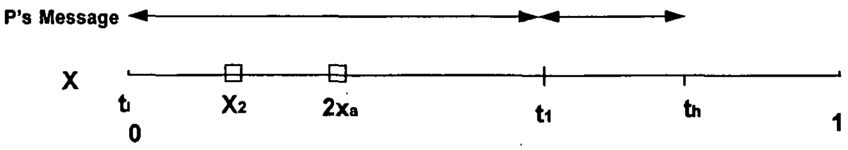


FIGURE 5

Equilibria under Incomplete Information



(a) Unitary Regime



(b) Agent-Veto Regime

☐ = F's Equilibrium Proposal

and *F* believes it, *F* will propose $x = 2x_a$, which is the best proposal that *F* can make to obtain *A*'s support. Thus, if $2x_a$ is sufficiently attractive to *P* relative to what *P* obtains by bluffing, *P* has an incentive to tell the truth when $t \in [2x_a, t_h]$. That is, if *P*'s preferences diverge too much from those of *A*, *P* will reveal its type and accept $2x_a$, because it fears that *F* may offer a proposal even worse than $2x_a$. The following proposition formally states this result:

PROPOSITION 2 (THE INFORMATIVE EQUILIBRIUM). *When* A*'s ideal point is relatively small (i.e.,* $x_a \le \bar{x}_a$*), the following constitutes an equilibrium:*[12]

1. P *rejects* F*'s proposal if it makes it strictly worse-off than the status quo outcome;* P *accepts it, otherwise.* A *has a similar veto strategy.*
2. P *sends a truthful message that its type is either in* $[0, t_1]$ *or* $[t_1, t_h]$*, where*

$$t_1 \equiv 2x_a + x_2 = 1 + 4x_a - \sqrt{1 + 4x_a^2}.$$

3. F*'s updated beliefs are consistent with* P*'s equilibrium signaling strategy.*
4. *If the message indicates* $t \in [0, t_1]$*,* F *proposes*

$$x_2 = \frac{1 + t_1 - \sqrt{t_1^2 - 2t_1 + 4}}{3}.$$

If the message indicates $t \in [t_1, t_h]$*,* F *proposes* $2x_a$*.*

If x_a *is sufficiently large, i.e.,* $x_a > \bar{x}_a$*, only a pooling equilibrium exists in which* F *makes a proposal based on its prior beliefs, that is,* $\pi^* = x_1$ *in equation 1.*

The informative equilibrium is also illustrated in Figure 5. It is straightforward to see that *P* has no incentive to deviate from the equilibrium signaling strategy. When $t \in [0, t_1]$, *P* has a choice to tell the truth or lie. If *P* tells the truth, it expects *F* to propose x_2. If it lies, it expects *F* to propose $2x_a$. Those types in $[0, t_1]$ prefer x_2 to $2x_a$, because $t \le t_1 = 2x_a + x_2$. Thus *P* has no incentive to lie when $t \in [0, t_1]$. Using a similar argument, I can show that *P* has no incentive to lie when $t \in [t_1, t_h]$.

The probability ρ_d of ratification in the informative equilibrium is

$$\rho_d = \frac{t_h - x_2}{t_h},$$

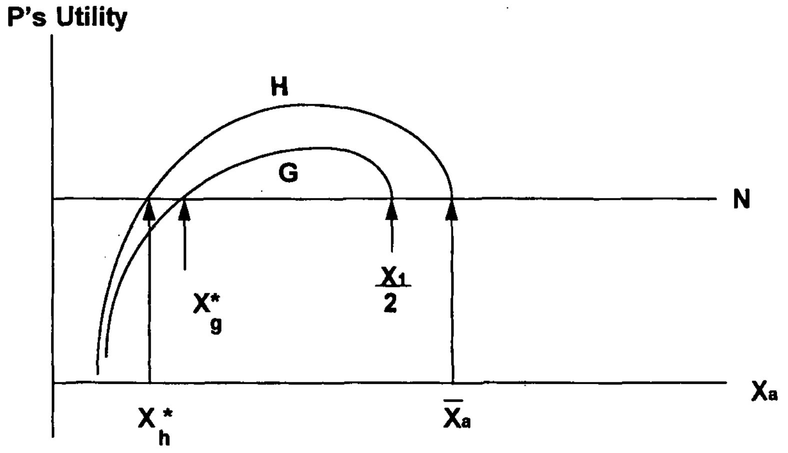
because only types in $[0, x_2]$ will reject *F*'s equilibrium proposal. Since x_2 is increasing in x_a, we see that the probability of ratification actually increases as *A*'s ideal outcome becomes smaller and more opposed to *F*. This result seems to be inconsistent with another conjecture by Putnam that greater domestic constraints decrease the probability of ratification. According to Putnam, *P*'s domestic constraints become greater when x_a decreases, because the set of proposals that *A* prefers to the status quo—*A*'s win-set—becomes smaller.

According to Putnam, a country's small win-set makes its bargaining position too inflexible and extreme against the foreign country, leaving little room for agreement. Putnam, however, does not consider how the foreign country will rationally respond to a smaller win-set. In my model, even with a smaller win-set, the foreign country will make a proposal that *A* will accept because it likes the status quo even less. At the same time, the probability of ratification increases because a smaller x_a forces *F* to make a proposal that is acceptable to more types of *P*. Thus we find that the relationship between the size of a country's win-set and the probability of ratification cannot be established without knowing players' equilibrium response to changes in the size of the win-set.

To evaluate *P*'s incentive to use the agent veto, I calculate *P*'s expected utility *H* in the informative equilibrium:

FIGURE 6

Unitary Versus Agent-Veto Regimes with Incomplete Information



$$H \equiv -\int_0^{x_2}\left(q-\frac{t}{2}\right)^2\frac{1}{t_h}\,dt-\int_{x_2}^{t_1}\left(x_2-\frac{t}{2}\right)^2\frac{1}{t_h}\,dt-\int_{t_1}^{t_h}\left(2x_a-\frac{t}{2}\right)^2\frac{1}{t_h}\,dt,$$

where the bargaining outcome is q in the interval $[0, x_2]$, x_2 in the interval $[x_2, t_1]$ and $2x_a$ in the interval $[t_1, t_h]$. Differentiating with respect to x_a yields

$$\frac{\partial H}{\partial x_a} = -2\int_{x_2}^{t_1}\left(x_2-\frac{t}{2}\right)\frac{\partial x_2}{\partial x_a}\frac{1}{t_h}\,dt-4\int_{t_1}^{t_h}\left(2x_a-\frac{t}{2}\right)\frac{1}{t_h}\,dt.$$

The sign of this derivative is indeterminate because the signs of the integrands, $(x_2 - t/2)$ and $(2x_a - t/2)$, depend on the values of t. A change in x_a has a positive effect on types of P in $[2x_2, t_1]$ and $[4x_a, t_h]$ but a negative one on those in $[x_2, 2x_2]$ and $[t_1, 4x_a]$. Numerical analysis shows that as x_a increases, H first increases and then decreases. As x_a increases, the bargaining outcomes in the informative equilibrium become more attractive to more types of P, thus increasing P's expected utility. However, as x_a approaches $t_h/2$, those outcomes become too large for most types of P and as a result, P's expected utility decreases.

Figure 6 draws P's expected utility under the agent-veto regime as a function of x_a. It is represented by H for $x_a \leq \bar{x}_a$, and N for $x_a > \bar{x}_a$. When $x_a > \bar{x}_a$, P's expected utility equals N because only a pooling equilibrium exist in that case.

The Choice of Domestic Bargaining Regime

The choice of domestic bargaining regime affects the international bargaining equilibrium. Thus, if we compare P's expected utilities in the agent-veto and unitary equilibria, we can gain insight into P's incentive to choose one regime over the other.

Figure 6 shows that when x_a is between x_h^* (at which H intersects N) and $\bar{x}_a$, P's expected utility is strictly higher under the agent-veto regime than under the unitary regime. Therefore, P will grant veto power to A only if $x_h^* < x_a < \bar{x}_a$.

Let us interpret this result in terms of P and A's preferences. First, A's ideal point must lie within the domain $[0, t_h/2]$ of the distribution of P's ideal points.[13] Moreover, since $x_h^* < t_h/4 < \bar{x}_a$, as Figure 6 shows, we can say that P will grant veto power to A if A's ideal point is closer to P's expected ideal point $E(x_p)$ which is $t_h/4$. The average distance between x_a and x_p is the minimum at $x_a = t_h/4$ and continuously increasing in x_a on both sides of the minimum. Thus veto power will be granted to A if, on average, A's ideal point is close to P's.

Proposition 3. *The principal will give veto power to its agent only if their preferences are similar, that is, the distance between their ideal points is small.*

A numerical example can provide more insight into the kinds of A to whom P wants to grant veto power. Suppose that P's ideal point x_p is uniformly distributed over the interval $[0, .4]$ with mean $E(x_p)$ of .2. Numerical analysis shows that in this case, $x_h^* = .093$ and $\bar{x}_a = .224$. Thus, P will grant veto power if A's ideal outcome x_a lies in the interval (.093, .224). Since $x_h^* < E(x_p) < \bar{x}_a$, A's ideal outcome must be relatively close to P's expected ideal outcome. The results also show that among the acceptable agents, relatively more of them have hard-line positions against F in the sense that their ideal outcome is farther from F's ideal outcome than from P's expected ideal outcome; that is, $E(x_p) - x_h^* > \bar{x}_a - E(x_p)$.

In the example above, A's veto power makes P strictly worse-off only if x_a is very small relative to $E(x_p)$, that is, $x_a < x_h^* = .093$. P is indifferent between the two regimes when $x_a \geq \bar{x}_a$. Thus, unless A's ideal outcome is very close to the status quo, P does not prefer the unitary regime to the agent-veto regime.

The attractiveness of the agent veto to P reflects two factors: (1) the veto's effect on the range of proposals that F can make—called the *range reduction effect*—and (2) its effect on the amount of information transmitted to F.

To isolate the range reduction effect, consider a pooling equilibrium under the agent-veto regime. Although I characterize only the informative equilibrium when $x_a \leq \bar{x}_a$ in proposition 2, a pooling equilibrium also exists. P's expected utility in that pooling equilibrium is G when $2x_a < x_1$ and N when $2x_a \geq x_1$ (see the proof of proposition 2 in the Appendix). As Figure 6 shows, even if no information is transmitted, the range reduction effect of A's veto power can be ex ante beneficial to P, because $G > N$ when $x_g^* < x_a < x_1/2$. Since $E(x_p) = t_h/4 > x_1/2$, this means that to receive veto power, A must have an ideal outcome that is, on average, farther from F's ideal outcome than P's ideal outcome is but not too far. A's ideal outcome must also be relatively close to P's expected ideal outcome. When A's ideal outcome is in that range, its veto power induces a foreign proposal that is better for most types of P. This range reduction effect is an incomplete information version of the benefit of a domestic constraint in two-level games.

Unlike the complete information version, however, the alignment of preferences between P and A matters when F has incomplete information about P's preferences. In the incomplete information case, A's veto power provides additional benefits to P, because it allows information to be transmitted between P and F. This informational effect is captured by the difference between F and G. Since $H \geq G$, P is willing to give veto power to more types of A than what the range reduction effect alone allows.

CONCLUSION

Putnam's conjecture that negotiators can benefit from their domestic constraints has implications for the design of domestic bargaining regimes in two-level games. If domestic constraints are as beneficial as Putnam suggests, negotiators have an incentive to impose domestic constraints on themselves before negotiating with their foreign counterparts. To examine this issue more closely, I develop a model of two-level games in which a negotiator decides whether to impose a domestic constraint on itself by granting veto power to an agent before negotiating with a foreign country.

I show that the negotiator's decision to create a domestic constraint depends in large part on whether or not the foreign country has information about the negotiator's preferences. If the foreign country has complete information, the negotiator will always grant veto power to the agent. Given the foreign country's knowledge of its preferences, the negotiator does not have much leverage without the agent veto.

When the foreign country has incomplete information about the negotiator's preferences, the negotiator will not always grant veto power. If the agent's preferences diverge too much from the negotiator's, the negotiator will choose the unitary regime where the agent has no veto authority. However, I also show that the range of agent preferences acceptable to the negotiator is surprisingly large. That is, the negotiator retains strong preference for the agent veto even when the foreign country has incomplete information.

The attractiveness of the agent veto to the negotiator comes from its range reduction and informational effects. The agent's veto power reduces the range of proposals that the foreign country can successfully make. If the agent's ideal point is not too far from a negotiator's ideal point, the negotiator benefits from this range reduction effect because it may induce the foreign country to make a proposal more favorable to the negotiator. The informational effect of the agent veto refers to better information transmission between the negotiator and the foreign country. By granting veto power to the agent, the negotiator can transmit more information in equilibrium to the foreign country and capture informational gains that would be lost in the absence of the agent veto. These informational gains give the negotiator an additional incentive to grant veto power to the agent.

The strategic granting of veto power to an agent is not limited to the case of international negotiations. The House-Senate bargaining in Congress also has this institutional feature. Before final passage, the House and the Senate form a conference committee to reconcile differences in bills. Representing the interests of the House floor, the Speaker of the House chooses House conferees. Thus the House floor is a principal and House conferees are its agents. Explaining the choice of conferees is an important issue in the congressional literature (Krehbiel 1991). Some have argued that conferees enjoy certain procedural advantages in bargaining with the Senate, such as veto power (Shepsle and Weingast 1987). If so, then the question of who goes to conference may be construed as asking to whom the House floor chooses to give veto power.

The model presented here predicts that given restrictive conference procedures, the House floor will choose non–preference outliers, that is, members with preferences similar to the House floor. This is consistent with Krehbiel's (1991) empirical finding that controlling for other influences, conferees tend to be non–preference outliers.[14] Moreover, the model explains why and how legislators' preferences affect the choice of conferees by the House floor. In bargaining with the Senate, the House may reap informational gains by using conferees as a means to reduce the Senate's uncertainty about House preferences. This device, however, is not beneficial for the House floor if the conferees have preferences too divergent from those of the floor.

APPENDIX

The Definition of a Sequential Equilibrium

In the definition given below, A has veto power. An equilibrium for the case in which A has no veto power can be defined without E2 and without $\alpha_a^*(x)$ in the conditional probability in E5.

A *sequential equilibrium* consists of strategies $\langle m^*, \alpha_p^*, \alpha_a^*, \pi^*\rangle$ and F's beliefs μ^* about t satisfying E1–E5:

E1. For all $x \in X$, $\alpha_p^*(x, t) = \begin{cases} 1 & \text{if } u_p(x, t) \geq u_p(q, t) \\ 0 & \text{otherwise.} \end{cases}$

E2. For all $x \in X$ and $t \in T$, $\alpha_a^*(x) =$

$$\begin{cases} 1 & \text{if } u_a(x) \geq u_a(q) \\ 0 & \text{otherwise.} \end{cases}$$

E3. For all $t \in T$, $m^*(t) \in argmax_{\hat{s} \in S} \begin{cases} u_p(\pi^*(\hat{s}), t) \\ u_a(\pi^*(\hat{s})). \end{cases}$

E4. If $m^*{-1}(s) \neq \emptyset$, then $\mu^*(\cdot, s)$ satisfies Bayes' Rule according to $\pi^*(\cdot)$ and prior belief $f(\cdot)$.

E5. For all $s \in S$, $u_f(\pi^*(s)) \geq u_f(q)$, and

$$\pi^*(s) \in argmax_{x \in X} \beta(x|s) u_f(x) + (1 - \beta(x|s)) u_f(q),$$

where $\beta(x|s)$ is the conditional probability that x is accepted,

$$\beta(x|s) = \int_T \alpha_p^*(x, t) \alpha_a^*(x) \mu^*(t, s)\, dt.$$

PROPOSITION 1. *Under the unitary regime, there can be only one elicited proposal in a sequential equilibrium.*

Proof. Suppose to the contrary that two proposals, say a_1 and a_2, are elicited in some equilibrium.

Let $a_2 > a_1$ and P's preference ordering be denoted by $>_p$, so $a_1 >_p a_2$ if P prefers a_1 to a_2. Then, $a_2 >_p a_1$ if $x_p > a_1 + a_2/2$ or $t > a_1 + a_2$. Since $a_1 + a_2$, $t \geq a_2$ is willing to accept both a_2 and a_1, but prefers not to elicit a_2 because $a_1 >_p a_2$. Thus, when F is supposed to propose a_2, it believes that t is not in $[a_2, a_1 + a_2)$. Given its beliefs that $t < a_2$ or $t \geq a_1 + a_2$, proposing a_2 cannot be optimal for F because proposing $a_2 + \epsilon$ has the same probability of being accepted by P as a_2 but gives higher utility to F. This contradicts the definition of a sequential equilibrium. QED

F's Problem in a Pooling Region.

When F believes that t is in some subset, say $[\alpha, \beta]$, of T, its proposal problem is more complicated than the complete information case. In this section, I derive F's optimal proposals x in that pooling region. If $x > \beta$, the proposal is rejected with probability 1 and $x < \alpha$, the proposal is accepted with probability 1. If $\alpha \leq x \leq \beta$, the probability of the proposal being accepted is $(\beta - x)(\beta - \alpha)$. Thus F's expected utility when it makes a proposal $(x, 1)$ in the pooling region $[\alpha, \beta]$, is

$$Eu_f(x) = \begin{cases} -1 & \text{if } x > \beta \\ K(x) \equiv -\left(\dfrac{\beta - x}{\beta - \alpha}\right)(x-1)^2 & \\ \qquad -\left(1 - \dfrac{\beta - x}{\beta - \alpha}\right)1 & \text{if } \alpha \leq x \leq \beta \\ -(x-1)^2 & \text{if } x < \alpha. \end{cases}$$

To find the optimal x when $x \in [\alpha, \beta]$, I rewrite $K(x)$ as

$$K(x) = \frac{x^3 - (2 + \beta)x^2 + (2\beta)x - \beta + \alpha}{\beta - \alpha}.$$

The optimal x maximizes $K(x)$ in $[\alpha, \beta]$ and depends on the shape of $K(x)$ in that interval. Since $K(0) = -1$, $K(\alpha) = -(\alpha - 1)^2$, and $K(\beta) = -1$, it is straightforward to show that $K(0) = K(\beta) < K(\alpha)$. To determine whether $K(x)$ can be larger than $K(\alpha)$ in (α, β), differentiate $K(x)$ with respect to x, which yields

$$\frac{\partial K}{\partial x} = \frac{3x^2 - 2(2 + \beta)x + 2\beta}{\beta - \alpha}.$$

It follows that

$$\frac{\partial K}{\partial x}(0) = \frac{2\beta}{\beta - \alpha} > 0, \quad \frac{\partial K}{\partial x}(\beta) = \frac{\beta^2 - 2\beta}{\beta - \alpha} < 0,$$

$$\frac{\partial K}{\partial x}(1) = \frac{-1}{\beta - \alpha} < 0,$$

and

$$\frac{\partial K}{\partial x}(2) = \frac{4 - 2\beta}{\beta - \alpha} > 0.$$

Thus $K(x)$ has a local maximum between β and 0 at

$$x = x^* = \frac{2 + \beta - \sqrt{\beta^2 - 2\beta + 4}}{3}.$$

If $x^* \geq \alpha$, the optimal x in $[\alpha, \beta]$ is x^*, and if $x^* < \alpha$, the optimal x is α. The sign of $\alpha - x^*$ is the opposite sign of

$$\frac{\partial K}{\partial x}(\alpha) = \frac{3\alpha^2 - 4\alpha - 2\beta\alpha + 2\beta}{\beta - \alpha}.$$

PROPOSITION 2.

a. If $x_a \leq \bar{x}_a \equiv \dfrac{\sqrt{4 - 2t_h + th^2} - 2 + 2t_h}{6}$,

the unique Pareto-optimal equilibrium is characterized by

$$m_p^*(t) = \begin{cases} s_1 & \text{if } t \in [0, t_1] \\ s_2 & \text{if } t \in [t_1, t_h], \end{cases}$$

$$\mu^*(s, t) = \begin{cases} U[0, t_1] & \text{if } s = s_1 \\ U[t_1, t_h] & \text{if } s \neq s_1, \end{cases}$$

and

$$\pi^*(s, \mu^*(s, t)) = \begin{cases} x_2 & \text{if } s = s_1 \\ 2x_a & \text{if } s \neq s_1, \end{cases}$$

where

$$x_2 = \frac{1 + t_1 - \sqrt{t_1^2 - 2t_1 + 4}}{3}$$

and

$$\begin{aligned} t_1 &\equiv 2x_a + x_2 \\ &= 1 + 4x_a - \sqrt{1+4x_a^2}. \end{aligned}$$

b. *If $\bar{x}_a < x_a \leq t_h/2$, only a pooling equilibrium exists in which $\pi^* = x_1$ in equation 1.*

Proof of a. First, I show that P does not have an incentive to deviate from the equilibrium signaling strategy. If $t \in [0, t_1]$ deviates and sends s_2, P expects F to propose $2x_a$. Types in $[0, x_2]$ are indifferent between x_2 and $2x_a$ because they will reject both. But types in $[x_2, t_1]$ prefer x_2 to $2x_a$ because $t_1 = 2x_a + x_2$ represents the type that is indifferent between x_2 and $2x_a$. Thus no type in $[0, t_1]$ strictly prefers to deviate. Likewise, types in $[t_1, t_h]$ do not have an incentive to deviate and send s_1 because they prefer $2x_a$ to x_2. Given P's equilibrium signaling strategy, F's beliefs are rational.

It remains to show that F's equilibrium proposals are optimal, given its equilibrium beliefs. When F receives $s \neq s_1$, it believes that $t \in [t_1, t_h]$ and proposes $2x_a$, which is the best outcome for F given that belief. When F receives s_1, it believes $t \in [0, t_1]$. Because of A's veto power, F will not propose any x greater than $2x_a$. If F proposes some $x \leq 2x_a$, its expected utility, given that belief, is

$$-\left(\frac{t_1 - x}{t_1}\right)(x - 1)^2 - \left(1 - \frac{t_1 - x}{t_1}\right)(q - 1)^2,$$

where $((t_1 - x)/t_1)$ is the probability of x being accepted. A unique interior solution to this maximization is x_2. Since x_2 is always smaller than $2x_a$, it is F's optimal equilibrium proposal.

Second, we need to show that the informative equilibrium is the unique Pareto-optimal equilibrium. This will be shown in two steps: (1) there is no sequential equilibrium in which more information is revealed than the informative equilibrium, and (2) the informative equilibrium Pareto-dominates a pooling equilibrium.

Using the same technique as in proposition 1, we can show that there can be only one elicited proposal in the interval $[0, 2x_a)$. This implies that there are at most two elicited proposals with $2x_a$ being one of the two. The informative equilibrium has two elicited proposals, x_2 and $2x_a$, so no other sequential equilibrium has more information revealed.

The only other sequential equilibrium is then a pooling equilibrium. F's equilibrium proposal in the pooling equilibrium is $w \equiv \min\{x_1, 2x_a\}$. Thus P's expected utility in the pooling equilibrium is G when $2x_a < x_1$ and N when $2x_a \geq x_1$. To see this, when $2x_a < x_1$, F knows that x_1 will be rejected by A and instead, offers $2x_a$. Then, if P's type is in $[0, 2x_a]$, P will reject $2x_a$; otherwise, P will accept it. Thus P's expected utility in this case will be G, the same as that in the complete information case with the agent veto. When $2x_a \geq x_1$, knowing that A will accept x_1, A will propose it as if there is no agent veto. Thus P's expected utility is N, the same as that in the unitary equilibrium.

Figure 6 shows that P's expected utility is not worse off in the informative equilibrium than in the pooling equilibrium, since $H > G$. Numerical analysis confirms that $H > G$ in general.

F also prefers, ex ante, the informative equilibrium to the pooling equilibrium. To show this, let C_i be F's expected utility in the informative equilibrium where

$$C_i = -\int_0^{x_2}(q - 1)^2\frac{1}{t_h}\,dt - \int_{x_2}^{t_1}(x_2 - 1)^2\frac{1}{t_h}\,dt - \int_{t_1}^{t_h}(2x_a - 1)^2\frac{1}{t_h}\,dt.$$

C_p is F's expected utility in the pooling equilibrium and can be expressed as a function of w:

$$C_p = -\int_0^{w}(q - 1)^2\frac{1}{t_h}\,dt - \int_{w}^{t_h}(w - 1)^2\frac{1}{t_h}\,dt.$$

Numerical analysis shows that $C_i \geq C_p$. Intuitively, F is better off in the informative equilibrium than in the pooling equilibrium because F's proposal is more likely to be accepted in the former, due to better information transmission. Therefore, the informative equilibrium Pareto-dominates the pooling equilibrium.

Proof of b. No information is transmitted in this equilibrium, so F in equilibrium makes a proposal based on its prior beliefs. When $x_a > \bar{x}_a$, however, $x_a > x_1/2$ because $\bar{x}_a > x_1/2$, so $\pi^* = x_1$, which is acceptable to A. QED

Notes

I would like to thank David Baron, David Epstein, Keith Krehbiel, and Harrison Wagner for helpful suggestions.

1. Putnam's idea that the domestic ratification process can be used as a bargaining advantage originates with Schelling (1960). According to Schelling, the use of a bargaining agent should be understood as a negotiator's attempt to commit to its preferred bargaining position. For example, "the use of a professional collecting agency by a business firm for the collection of debts is a means of achieving unilateral rather than bilateral communication with its debtors and of being therefore unavailable to hear pleas or threats from the debtors" (pp. 142). The creditor using a collecting agency to commit to a uncompromising stance against the debtor corresponds to a negotiator granting veto power to another domestic actor in international negotiations.

2. Pahre (1994) is an exception, but he only looks at the complete information case and does not consider the case of incomplete information.

3. For other models of two-level games, see Iida 1993; Mayer 1992; Mo 1994.

4. Lax and Sebenius (1991) and Martin (1994) examine other sources of uncertainty in their bargaining models with internal conflict. Lax and Sebenius model an agent who is uncertain about the principal's preferences, while Martin's agent has private information that the principal does not have. These authors, however, do not address the signaling consequences of the agent's veto power.

5. Alternatively, P can choose a regime after P's type is revealed. In this case, the choice of a regime can signal P's type. For a similar problem in a legislative setting, see Gilligan and Krehbiel 1989.

6. Since S is continuous, an equilibrium in which a segment of T is truthfully signaled to the foreign country is not ruled out.

7. The formal statement of this equilibrium concept is given in the Appendix.

8. As an anonymous referee for this journal observes, if we relax the assumptions that both P and A prefer a negotiated outcome to the status quo or that F's ideal point is not acceptable to P and A in comparison to the status quo, P's incentive to give veto power to A weakens. For example, when $x_a < q$, A will always reject F's proposal that P may want to accept, so P will not give veto power to A in this case. If t and $2x_a$ is greater than 1 (i.e., F's ideal point is acceptable to P and A), P does not need an agent to influence international bargaining outcome because F will always propose $x = 1$ which both A and P will accept.

9. This result corresponds to Pahre 1994, hypoth. 13.

10. As part of the 1988 omnibus trade bill, Congress sought to reduce the role of the president in Section 301 bargaining (Bello and Holmer 1990). Under the old law, the president had discretion in deciding whether to retaliate against unfair foreign trade practices. But Congress wanted to make retaliation automatic in some cases, revoking the president's veto power. For example, when a foreign practice in question violates U.S. rights under an existing trade agreement, the president must now retaliate against that foreign country. The fact that Congress initially permitted a presidential veto over Section 301 decisions but later decided to limit the veto shows that Congress does not always favor giving veto power to another domestic actor.

11. This assumes that $2x_a < t_h$. The case of $2x_a \geq t_h$ is uninteresting because A's veto power is irrelevant to F's proposal problem and the game is identical to the unitary regime case.

12. At $\bar{x}_a$, $t_1 = t_h$. See the Appendix for the closed form expression for $\bar{x}_a$.

13. The first inequality is based on numerical analysis. The second inequality can be derived from the sign of

$$\bar{x}_a - \frac{t_h}{4} = \frac{-4 + t_h + 2\sqrt{4 - 2t_h + t_h^2}}{12},$$

which is positive.

14. Krehbiel also finds expertise effects; that is, conferees tend to be specialists. My model does not capture these expertise effects. In my model, all potential conferees can be thought of as having a common level of expertise, because their preferences are assumed to be well known. One can argue that as Congress members build their policy reputation in Congress, there is less uncertainty about their preferences.

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FULLY PROPORTIONAL REPRESENTATION

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Even though voters can have complex preferences over multiple candidates or parties, all extant electoral systems provide political representation based solely or primarily on voters' first preferences. I present a new concept of proportional representation that takes account of voters' preferences over the full list of alternatives—fully proportional representation (FPR)—and schemes for its implementation. I outline a "pure" FPR scheme, but because this scheme would have several undesirable features when used by real voters, I also discuss modifications that account for these difficulties. Although there are a variety of interpretations of the role played by voting in democracy, several can be shown to suggest FPR as a normative ideal. Fully proportional representation provides us with new ways to conceptualize existing electoral systems, a new standard against which alternative systems can be evaluated, and several feasible alternatives for approximating this new ideal.

Duncan Black (1958) defines *proportional representation* as an attempt to select a political assembly that is a "reflexion" of the "shades of political opinion" in society as a whole. Most electoral systems in use today for national elections assume that the political opinion of a voter is completely embodied in a vote for just one candidate or party, and all systems in use assume that this single preference is the most important. For elections to single offices—or in single-member districts—this is often reflected by the use of simple plurality rule. Many alternatives to simple plurality rule have been suggested and analyzed in the literature of social choice theory (e.g., the Borda rule, the Copeland rule, approval voting, the Nanson rule, antiplurality voting). Although there is a great deal of disagreement about the best of these systems (and, of course, no system can be perfect), many social choice theorists find simple plurality rule to be worse, by many criteria, than other systems. The social choice literature comparing these types of voting systems is enormous (Kelly 1991). The same is not true about multiple-winner systems, particularly schemes where proportional representation is the implicit or explicit goal. Not only are there no alternatives to first-preference-based proportional representation (PR) systems in use, but few have been discussed in the academic literature (exceptions include Black 1949, 1958; Brams 1987; Chamberlin and Courant 1983; Dodgson 1884b; Dummett 1984; Myerson 1993; Rapoport, Felsenthal, and Maoz 1988a, 1988b).[1] This lack of discussion about alternative multiple-winner systems is no doubt fueled by the fact that in most of these cases, the authors reject the alternative systems they suggest.

The present article is motivated in part by the assumption implicit in the "single-winner" voting system literature: voters' political preferences are more complex than first choices alone and election results can reflect that complexity. Our focus on first preferences has led us to think about proportional representation in the wrong way. There are different (and, I will argue, *better*) answers to the questions, Proportional to what? and Representation of what?

The concern is not just theoretical. In the United States, for instance, there have been important public and legal debates about appropriate goals for the political representation of racial minorities, and the best electoral mechanisms for achieving these goals. Perhaps sparked by the writings of (and public controversy surrounding) Lani Guinier, a mechanism called "cumulative voting" has received much attention and has even been court-mandated in a recent case (*Cane* v. *Worcester County*, 847 F.Supp. 369). Guinier's defense of cumulative voting is that "it allows voters to organize themselves on whatever basis they wish" (1994, 15; see also Engstrom 1992). I shall provide arguments for more specific but very similar objectives and agree that cumulative voting will better achieve these goals than the systems most widely used in the United States. I also argue, however, that there are even better mechanisms that should be considered if these goals are taken seriously. Such concerns are not limited to the U.S. context, of course, and would also inform the debates over electoral reform in Japan, South Africa, Britain, and elsewhere. The systems discussed here are designed to provide voters with the greatest possible control over how they will be represented.

The basic principle behind these systems was first tentatively suggested and abandoned by Charles Dodgson (Lewis Carroll) (1884b)[2] and then suggested and rejected in different form by Black (1958). This principle is as follows: if m candidates are to be elected, voters should be segmented into m equal-sized coalitions, each of which is assigned a representative, such that the preferences of voters are as closely as possible reflected by the representatives of their segment. There are a number of definitional ambiguities and implementational difficulties in this principle that make the realization of such a system less than straightforward. Nevertheless, several such systems are feasible and are discussed here. Well-known systems like single transferable vote, single nontransferable vote, cumulative voting, and party-list PR can be characterized as modifications of this general type of system. Systems that take account of voter preferences over all possible alternatives are herein given the label *fully proportional representation* (FPR).

I am not advocating the adoption of FPR in actual legislative elections. However, I am advocating further discussion of research into the ideal of FPR. My intention is to reexamine the basic foundations of representative democracy, not to restructure any particular democratic institutions. By returning to first principles (through an abandonment of first-preference biases), I show that FPR has properties that have been claimed desirable by other scholars in their defenses of alternative systems and that it is the system that most closely fits several normative ideas about what it is that an electoral system ought to do.

I do not challenge another first principle of all extant representation systems, namely, that voters care about (or at least should be given control over) the selection of only a small portion of the assembly. In a few cases, like the U.S. Senate, voters have direct control over the selection of multiple representatives; in most cases, voters directly affect only the selection of a single representative. Only in limited cases (e.g., block voting for some local councils in the United States)[3] do voters directly affect the selection of an entire assembly. The resulting assemblies in such cases are generally one party or one race and generally viewed as "unrepresentative." I shall return to this discussion, but the reader should be aware that because my focus is solely on representation herein, I leave this assumption largely unchallenged throughout.

I begin with a discussion of early attempts by Black and Dodgson to develop schemes for FPR, generalize their idea to describe a "pure FPR" scheme, describe a first approximation of FPR using ordinal ballots, and briefly outline several normative justifications of the FPR concept. Because neither pure nor ordinal FPR provides a reasonable alternative for actual elections, I then discuss how several alternative representation systems, extant and theoretical, can be characterized as modifications or approximations of pure FPR. Finally, I compare FPR to alternative systems recently offered by other scholars.

DUNCAN BLACK'S "TRIAL SCHEME"

In his first writings on proportional representation, Dodgson (1884b) asserted that a representation system should find the coalitions in the electorate that would have formed if the voters had the necessary time and information. He abandoned the idea when he discovered the instabilities of coalition formation—another permutation of the now well-known majority cycling problem. It is instructive, however, to examine a similar "trial scheme" for PR that was suggested by Duncan Black (1958, 76–80). This scheme is not terribly useful for practical purposes, as it applies only to electorates with one-dimensional single-peaked preferences. Black himself rejected his scheme for this and other reasons I shall discuss. This discussion is useful for pedagogical purposes, however, because his scheme is an easily understood example of FPR in a special case.

Black imagines a set of alternatives that can be arranged along a single spatial dimension, providing voters with single-peaked preferences. In one of his examples, there are 99 voters (v_1–v_{99}) and 9 candidates (a_1–a_9), three each from each of three parties. The voters and the parties (Labour, Liberal, and Conservative in the example) can both be arranged on a single left-right dimension. He assumes that the positions of candidates a_1 through a_9, correspond to the ideal points of voters v_1, v_{17}, v_{33}, v_{34}, v_{50}, v_{66}, v_{67}, v_{83}, and v_{99}, respectively. For instance, voter v_{50} likes Liberal candidate a_5 the most, Liberal candidates a_4 and a_6 next most, Labour candidate a_3 and Conservative candidate a_7 next most, and so on.

Assume three of these candidates are to be elected. The voters are divided into three segments of 33 voters each according to their relative positions (segment 1 contains voters v_1–v_{33}, segment 2 voters v_{34}–v_{66}; segment 3, voters v_{67}–v_{99}). The candidates elected are those preferred by the median member of each segment (a_2, a_5, a_8), these alternatives being the Condorcet winner in each case. This is a basic form of FPR: (1) voters are segmented into m equally sized and relatively homogeneous coalitions (as defined by their locations in the policy space), and (2) each coalition is assigned a representative (the first choice of its median voter).

The value of m (district magnitude) is an important determinant of the representation outcome. In particular, there are amonotonicities, in which one candidate may lose because too many seats are available. Although initially counterintuitive, this property is to be expected. Consider changing the value of m in Black's example. With $m = 1$, we will elect a Liberal candidate; with $m = 2$ (coalitions of 50 voters), one Labour and one Conservative; with $m = 3$ (coalitions of 33), one of each party; with $m = 4$ (coalitions of about 25), one Labour, two Liberal, and one Conservative. There may be some "natural" number of representatives ($m = 3$, perhaps, in this example) that will be difficult to determine in general and could be subject to strategic manipulation by lawmakers. Observing that amonotonicity is a natural and desirable phenomenon in the Black example should help us understand the source of amonotonicity in the more complicated examples.

Black ultimately abandoned this scheme, offering four objections:

1. Selected candidates may have lower Borda scores[4] than candidates not selected. This is the case for a_2 and a_8 in the example.
2. Candidates not selected may be preferred by a majority to candidates selected. This is the case for a_2 and a_8 in the example.
3. Selected candidates may be the first preference of no more than a single voter. This could be true of

any of the alternatives in the example. For instance, it might be the case that voters 1–16 prefer a_1 and voters 18–33 prefer a_3 but that a_2 is elected.

4. The scheme "depends only on the position of the peaks of the curves concerned and does not take into account the entire constellation of the curves." (p. 80)

None of these is a powerful objection. The first two are essentially the same objection and surely confound the objective of PR. If we accept them, we will elect three Liberal candidates (or the same one three times—there is no reason for multimember constituencies in this case). The point of PR is to elect candidates representative of the electorate's constituent segments. Giving Conservative voters control over the selection of the Labour candidate is unnecessary, because they already have control over the selection of their own candidate. Even PR schemes in use today do not have such features. The third objection is a bit surprising, coming from Black. When discussing single-winner voting systems, he goes to great pains to show that first-preference-based elections have a number of undesirable features. There seems little reason to apply different standards to the intrasegment selection of a representative than to the election for a single-member constituency. The fourth objection is also a bit surprising. The scheme *does* take account of voters' full preference curves by placing them in a policy space. The spatial conception of preferences—that utility declines continuously with distance from ideal points—is fundamental, for instance, to the derivation of Black's median voter theorem.

A much stronger objection is to the scheme's dependence on single-peaked preferences in the electorate. This assumption makes Black's discussion of the scheme no more than a pedagogical exercise (all Black intended in any case). Any practical suggestion for FPR must be able to process any possible distribution of preferences.

"PURE" FULLY PROPORTIONAL REPRESENTATION

Black's scheme can be generalized to one that provides representation for any possible distribution of preferences—*pure FPR*. The scheme is "pure" because it makes the unrealistic assumption that we know the extent to which each voter is "represented" or "misrepresented" by each potential representative. That is, it assumes we have accurate and detailed information about voter preferences. As this assumption is clearly false, I also discuss an approximation of pure FPR under which we assume that voters sincerely rank the candidates on an ordinal ballot. As this approximation is impractical in the face of even a few strategic voters, I discuss in subsequent sections the implications of further modifications to pure FPR.

The Pure Fully Proportional Representation Concept

Let us first establish some definitions. We assume that we can quantify the *misrepresentation* of voter v_a when represented by candidate x_i; call this μ_{ia}. Further assume that for reasons of voter equality, the scale of μ_{ia} across voters can be normalized and then compared. We could assume, for instance, that each voter's favorite representative provides $\mu_{ia} = 0$, each voter's least favorite provides $\mu_{ia} = M$, and all others provide μ_{ia} such that $0 \leq \mu_{ia} \leq M$. If given a utilitarian spin, this assumption may appear superficially to ignore the well-known problems with "interpersonal comparisons of utility." It is, however, essentially a "one person, one vote" principle, in which we take such interpersonal comparisons problems seriously: we recognize that voters vary in the intensity of their preferences but that we should count each voter only once. I shall discuss further normative justifications for this concept, noting at this point only that misrepresentation can be interpreted as equivalent to numerous undesirable quantities, such as "disutility" or "coercion."

Our objective in pure FPR is to find the set of m representatives, each associated with an equally sized constituency of (n/m voters), for which the total misrepresentation (summed across all voters), μ, is minimized. In effect, we want voters to define *for themselves* the group memberships or identities that they wish to have represented and to have maximum flexibility in doing so.

The most straightforward method for finding this optimal set of representatives is to examine every possible set of representatives. For each possible set, we must segment n/m of the voters with each representative so that μ is minimized. The simplest way to achieve this minimization involves two steps. First, each voter is assigned to the representative (in the set to be considered) who provides the lowest misrepresentation value for that voter. Indifferent voters can be distributed randomly. Second, if the segments are not equal in size (that is, each within one voter of the ratio n/m), their membership must be adjusted until they are equal.[5] This is done by shifting the voters that will suffer the least increase in misrepresentation from the shift. For example, if $m = 2$ and the set under consideration is $\{x_1, x_4\}$, then a voter for whom the difference in misrepresentation between x_1 and x_4 is .2 will be shifted before one for whom the same difference is .7. The latter voter, in turn, will be shifted before one that puts them first and last (where the difference will be 1.0). Voters that are equally good candidates for shifting can be shifted in random order. Determining the proper order of shifting is more tedious when $m > 2$, but the process is still straightforward.

Once we have m voter segments, each the same size (give or take one voter), we can measure μ for the configuration. We repeat the procedure for each possible set of representatives, and select the set that provides the minimum level of μ. Configurations that do poorly will be the ones where most of those voters assigned to a representative prefer someone else. This may or may not be reflected in a large number of "shifted voters," depending on whether or not the "someone else" is in the set under consideration.

One might ask, however, why we shift voters at all, given that they would prefer to be represented by someone else. At first glance this might seem to be in defiance of their vote, but it is quite the opposite. Voters are excluded from their "natural coalition" only when their votes are superfluous. Rather than "wasting" their vote, they are allowed to have influence in another coalition. This should not be an unfamiliar idea to electoral scholars, as it is exactly the motivation behind the transfer of surplus votes under the single transferable vote system.

Fully Proportional Representation with Ordinal Balloting

Pure FPR has a number of problems that require us to modify it for practical use. For the purposes of providing the most complete example of FPR possible, it serves here to concentrate on the special case in which voters are asked to rank the candidates from most preferred to least preferred (i.e., to provide ordinal rather than cardinal ballots). Our task is then to interpret these ballots in the "misrepresentation" framework and find the optimal set of representatives.

The easiest (but by no means the only) way to interpret such ballots is as interval expressions of misrepresentation levels. Each of the n voters, v_a, provides a ballot ranking each of the candidates, x_i. The rank of a particular candidate, x_i, on the ballot of a particular voter, v_a, is r_{ia}. The statement $r_{ia} = 1$ indicates that v_a has placed x_i first on the ballot. If the voter expresses indifference among two or more candidates, each is considered to be ranked at the average of the rankings those candidates would occupy if listed separately. For instance, if candidates x_i and x_j are listed as tied for first, then $r_{ia} = r_{ja} = 1.5$. When a voter provides an incomplete list of candidates, we can make a similar assumption of indifference among unlisted candidates.[6] We can now simply define the misrepresentation of voter v_a when represented by candidate x_i as $\mu_{ia} = r_{ia} - 1$ and proceed as before. That is, our objective as before is to find the set of m representatives, each associated with a constituency of n/m voters, for which total misrepresentation is minimized. The mechanics of doing so are as I have described.

Note that since this definition of misrepresentation is essentially a reverse Borda score, ordinal FPR is a multiple winner extension of the Borda rule. That is, if we are electing only one representative ($m = 1$), then ordinal FPR selects the Borda winner. Furthermore, with few exceptions, the representatives elected when $m > 1$ will each be Borda winners for the constituency of n/m voters to which they are assigned. Many readers will anticipate that since the Borda rule is so manipulable as to be useless for most practical voting situations, ordinal FPR is as well. Nevertheless, it serves as a straighforward approximation of the pure FPR ideal from which we can develop an example and illustrate the concept. More pragmatic modifications of pure FPR that can be expected to work well even in the presence of manipulative voters will also be discussed.

An Example

Here is an example drawn from a situation outside party politics, in which preference orders were expressed under an entirely different (and ambiguous) choice mechanism. It is included only for the purposes of illustrating the mechanism that has been described and is not a presentation of experimental results.

The 33 voters in the example were students in an Oxford University graduate common room in 1991. The alternatives were six newspapers to which the common room might collectively subscribe: the *Daily Telegraph* (staunchly conservative), the *Times* (mildly conservative), the *Independent* (centrist/politically neutral), the *Guardian* (Labour/left), the *Financial Times* (business-oriented), and the *International Herald Tribune* (American). The situation resembled a party-political proportional representation situation in that the objective was to find a subset of these six newspapers that was most representative of the voters' preferences. Although it was clear (by almost any aggregation method) that if only one paper could be chosen, the *Independent* was the best choice, no one would have recommended that the group buy three copies. Similarly, although the *Times* was probably the second most preferred by the group as a whole, this may have been due only to its similarity to the *Independent*. We want a PR scheme that can detect (or ignore) the presence of a "clone." (Otherwise the system would provide parties with incentives to break into smaller and smaller factions.) If three or four are to be chosen, it is the task of the PR system to determine if there are implicit minority coalitions (e.g., "American," "business-oriented," or "Labour/left") of sufficient size to warrant the selection of a paper representing them.

The students were asked to rank the alternatives in order of preference. No decision mechanism was specified.[7] Some voters ranked all six alternatives, but most ranked fewer. One voter listed only a first choice. No voters expressed indifference on any pairs, probably because this option was not made explicit on the ballots. The ballots are listed in Table 1.

Table 2 shows the optimal solutions for m values (district magnitudes) of 1, 2, 3, and 4. In each case, the total misrepresentation for each of the possible sets has been shown. I have also shown one of the assignments of voters to papers that would provide that minimal level of misrepresentation, although it should be noted that in each multiple-winner case, there are several segmentations of the electorate that

TABLE 1

Ballots for Newspaper Preference

PREFERENCE	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33
1st	F	I	I	F	H	H	F	T	G	D	G	F	F	F	T	I	I	I	T	I	F	H	I	I	I	D	T	G	I	F	H	T	I
2d	T	T	T	T	I	F	T	F	I	I	D	H	I	G	—	D	H	H	D	H	G	T	D	T	T	T	D	D	T	T	F	I	G
3d	I	G	G	I	G	I	I	—	—	T	T	I	G	H	—	G	G	T	I	G	I	I	G	D	G	I	I	I	G	I	T	H	F
4th	—	D	—	G	—	—	H	—	—	—	—	G	H	T	—	F	T	G	H	—	—	G	T	—	—	G	G	T	D	—	—	G	T
5th	—	H	—	D	—	—	D	—	—	—	—	T	—	—	—	—	D	—	G	—	—	F	F	—	—	F	H	F	—	—	—	—	D
6th	—	F	—	H	—	—	G	—	—	—	—	D	—	—	—	—	F	—	F	—	—	D	H	—	—	H	F	H	—	—	—	—	H

Note: D = the *Daily Telegraph* (conservative), F = the *Financial Times* (business-oriented), G = the *Guardian* (Labour/left), H = the *International Herald Tribune* (American), I = the *Independent* (neutral/centrist), and T = the *Times* (mildly conservative). Voters were 33 graduate students at Lincoln College, University of Oxford, 1991. Voters were asked to list papers in order of preference. No voting system was specified. I am grateful to the Lincoln College MCR Committee for providing the anonymous ballots.

provide the same overall misrepresentation level. For the purposes of this example, I have treated the newspapers as if they were individual candidates, not parties. The mechanics of the FPR system are exactly the same in the party case, except that we must also consider the possibility of representative sets with repeated alternatives. For instance, with $m = 2$ in the newspaper example, we would also consider the possibility of selecting *two* copies of the *Independent* or the *Times*, as well as the pair of them individually.

For $m = 1$, the pure FPR system is equivalent to the Borda count. In this example, the *Independent* is the clear Borda winner. All of the advantages and disadvantages of the Borda rule are inherent in ordinal FPR.

For $m = 2$, the *Independent* is still chosen, but the second segment has the *Financial Times* as its representative paper. Note that in a conventional Borda ranking, the *Financial Times* would be fourth. Extending the Borda rule to the multiple-winner environment does not require merely selecting the top m

TABLE 2

Fully Proportional Representation in the Newspaper Example: Winners and Total Misrepresentation with Different Numbers of Segments

SEGMENT	MEMBERS	WINNER	MISREPRESENTATION
One Segment ($m = 1$)			
1	All	I	42
Two Segments ($m = 2$)			
1	1,4,6,7,8,9,11,12,13,14,15,21,26,28,30,31	F	17
2	2,3,5,10,16,17,18,19,19,20,22,23,24,25,27,29,32,33	I	9
Total	—	—	26
Three Segments ($m = 3$)			
1	3,5,16,17,18,20,23,24,25,29,33	I	1
2	2*,8,10,11*,15,19,22,26,27,28*,32	T	10
3	1,4,6,7,9*,12,13,14,21,30,31	F	4
Total	—	—	15
Four Segments ($m = 4$)			
1	2,3,9,16,17,24,25,29,33	I	1
2	1,4,7,12,13,14,21,30	F	0
3	8*,10,11,19*,23*,26,27,28	D	7
4	5,6,15,18*,20*,22,31,32*	H	5
Total	—	—	13

* These votes are "surplus" for the election of a preferred candidate and have been transferred.

ranked alternatives. In this case, the second-ranked alternative, the *Times*, appeals to largely the same subset of the electorate as does the *Independent*, only not as much. Ordinal FPR indicates that if two papers are to be chosen, the most liked general paper and the slightly specialized financial paper will best represent the voters as a whole, even though they are not by themselves the two most highly rated alternatives.

For $m = 3$, the *Independent*, the *Times*, and the *Financial Times* are chosen. With segment size now smaller, the FPR process reveals that there is a "conservative" minority large enough to warrant the selection of the *Times*. This judgment is further substantiated by the second-place finish of the set including the *Independent*, the *Financial Times*, and the *Daily Telegraph*. Other highly ranked sets indicate that there is also discernible minority support for the *Guardian* and the *International Herald Tribune* but that these segments are also noticeably weaker.

For $m = 4$, we have a tie. The tied sets both include the *Independent* and the *Financial Times*, but one includes the *International Herald Tribune* and the *Daily Telegraph*, and the other the *Times* and the *Guardian*. There is no irrefutable theoretical reason to select one set over the other. We might examine each alternative's (Borda) ranking in the electorate as a whole (a method that advises the selection of the latter set, because the *Times* and the *Guardian* are ranked third and fourth), or we might simply select one of the sets at random. The problem is really an artifact of the example's small scale (33 voters selecting four of six alternatives), which would occur with very low probability in a mass electorate. The example in Table 2 is for the former set (that containing the *International Herald Tribune* and the *Telegraph*). In this case we observe an amonotonicity relative to the solution for $m = 3$. That is, the *Times* is a winner when there are three "seats" but loses when another seat becomes available. This amonotonicity is of the same type as those observed in Black's schemes. Although initially counterintuitive, this is a desirable property of the system.

Finally, note that the total misrepresentation declines as the number of segments increases (Table 2). This is directly analogous to the well-known finding in electoral studies that "disproportionality" within an election district decreases as the district magnitude (m) increases (Rae 1971; Taagepera and Shugart 1989). It also provides some intuition for my earlier claim that FPR and conventional first-preference PR provide similar results as m grows large. Consider the case where $m = n$—where each voter has a personal representative. In this case, it is obvious that each should be represented by his first choice. Both FPR and PR will provide this result. It is more usually the case, however, that n/m (constituents per representative) is quite large (on the order of 10 to 500 thousand). It is for these more relevant electoral situations that FPR could be substantively important.

THE CASE FOR FULLY PROPORTIONAL REPRESENTATION

Although the egalitarian foundations of the system are presumably clear, FPR is based on a specific concept of "misrepresentation minimization" that has yet to be given normative substance or defended. Such a defense requires that we examine why democracies vote—that is, what institutions of representation are supposed to accomplish. It would be inappropriate to go into great detail on this issue here or attempt to resolve it, but I can show that several plausible answers to such questions offer misrepresentation minimization as an ideal. That is, several quite different plausible interpretations of democracy provide us with prima facie justifications for further examination of FPR.

Elsewhere I have discussed three interpretations of the democratic process, each providing a goal for democratic institutions to accomplish (Monroe 1994b). When applied to the study of single-winner voting systems, several intriguing conclusions can be drawn. First, rather than suggesting different institutions, all three of the interpretations suggest an ideal that can be equated to misrepresentation minimization. All other voting systems can be characterized as modifications, approximations, or even distortions of this ideal. The Borda rule is an approximation of this ideal based on ordinal ballots. Plurality rule[8] and approval voting[9] are two others, based on even simpler ballot structures. Second, the susceptibility of the naïve misrepresentation minimization in pure FPR to easy and undesirable manipulation makes it untenable for most political uses. Our goal then is to examine modified systems for those which are robust to manipulation, providing approximations of the results that would have been obtained under the pure system with sincere voters. The Borda rule is extremely bad against this standard, being not only easy to manipulate but also easy to manipulate away from ideal results. Approval voting, on the other hand, is strongly robust against such manipulation (see Monroe 1993 for specific results). This line of reasoning can be extended into the multiple winner context: just as misrepresentation minimization provides an untenable ideal to be approximated by alternative single-winner voting systems, pure FPR is an untenable ideal we must approximate with modified forms of representation systems. I turn now to the alternative justifications of pure FPR as an ideal.

The Utilitarian Interpretation of Democracy

Social choice theorists often assume that each possible alternative or candidate would, if selected, provide each voter with some level of cardinal utility. Further, the goal of political institutions including voting systems, is to maximize social utility—that is, to select the alternative for which the sum of individual utilities is the greatest. This is the *utilitarian interpretation* of democracy, an interpretation that justifies democracy as a method for the satisfaction of individual interests, an idea traceable at least to Bentham's *Principles of Morals and Legislation* of 1789 (Bentham 1941).

Under this interpretation, "misrepresentation" is equivalent to "disutility." In the single-winner ($m = 1$) context, we need only impose the constraint of voter equality (i.e., the range of utility from most preferred to least preferred is, or is counted as, identical for all voters). In the multiple-winner context, we seek to elect $m > 1$ representatives for an electorate with n voters. To recommend the pure FPR concept as described, we need further assumptions. Voter equality demands that we divide the electorate into m segments of equal size (n/m) and assign a candidate to each segment. Pure FPR follows if we further assume that voters receive utility only from the representative assigned to their segment. The extent to which this assumption is true depends, of course, on the nature of the representative body. While it is clearly a valid assumption for the selection of newspapers (I really only care about the one that I read), it may be less valid for a representative body that will subsequently make policy decisions. Note, however, that such an assumption underlies all of the electoral systems in use today in that each voter directly affects only the selection of a small subset of the assembly (usually a single representative), not the entire assembly.

Under this interpretation, the argument that FPR meets Dodgson's goal (i.e., that the representation system find those coalitions in the electorate that would have formed if the voters had the necessary time and information) can be illustrated. The competition among candidates to provide representation to the electorate is much the same as that envisioned by Tiebout among communities to provide citizens with local public goods (Tiebout 1956). Tiebout shows that the optimum level of provision for public goods can be approximated when these goods can be provided locally and citizens can move freely among communities seeking those that match their preferences. Pure FPR is, in a way, a special case of Tiebout's model. Here, we envision a community or coalition formation process where the coalitions each receive utility from the representative to which they are attached, where the coalitions must be of population n/m or 0, and where utility is transferable so that voters will switch places to raise social utility. Fully proportional representation is a method for finding this equilibrium, approximately, from voters' expression of preference on their ballots.

The solution is approximate because regardless of m, such a system is very complex for voters, as well as unlikely, in any case, to provide utility-maximizing results when manipulated. One approximation is that with ordinal ballots outline above. If voters provide only a list of alternatives, we might make any number of assumptions about the distribution of voter utility underlying the list. The simplest and a priori most justifiable assumptions are those of voter equality and preference equality. First, voters are considered to be equally intense about their preferences. Second, for each voter the difference in utility between the first preference and the second is assumed to be the same as that between the fourth and the fifth (or the n^{th} and the $(n + 1)^{th}$). If we are selecting a single winner, these assumptions lead to a recommendation of the Borda rule. Ordinal FPR follows for multiple winners. Note that different assumptions about voter utility would suggest different electoral systems. Most familiar electoral systems, for instance, can be characterized as modifications of ordinal FPR in which we assume that voters are concerned only about the difference in utility between their first preferences and all others.

Preference equality does have several strong arguments in its favor, however. It is just this assumption that underlies LaPlace's (1812) justification of the Borda rule (see also Black 1976) and the compelling update of this argument by Sugden, showing that the Borda rule would be chosen from behind a Rawlsian veil of ignorance by expected-utility maximizers (1981, 140–45). More conclusively, Saari (1990, 1994) has shown that any system that gives points to candidates according to their rank in preference orderings must have the same flaws as the Borda/preference equality system and more. Of course, the Borda rule (or by Saari's result, all point systems) and the ordinal FPR system that is based on it are both highly manipulable and unlikely to find social-utility maximizers in practice. Approval voting and FPR with approval balloting are the most promising systems when manipulation is possible and will be discussed at length.[10]

The Minimum-Coercion Interpretation of Democracy

Under the *minimum-coercion interpretation*, democracy is justified as a method for the maximization of individual liberty. Democratic outcomes are those that force the fewest people to act against their will. Democratic choices and the selection of choice mechanisms are difficult because people disagree. In the case of an election among candidates of equal a priori standing, a choice must be made even if people cannot reach a consensus. Once a choice has been made using some voting mechanism, the group has acted as if it had reached some level of agreement. Requiring citizens to abide by democratic choice (or at least expecting them to do so), then requires at least implicit coercion of those that originally disagreed. The appropriate voting system under this interpretation is then the one that minimizes the total extent of individual coercion required to make collective decisions. Contrasted with utilitarianism that seeks to satisfy wants, this "liberal" interpretation of democracy seeks to protect rights.

For my purposes here, however, the implications of these two interpretations are (or at least can be) the same. For this to be true we must interpret misrepresentation values to be indicators of "coercion", and assume, as before, that voters are equally intense about their preferences and that coercion is experienced by each voter only to the extent that the individual representative (not the assembly as a whole) fails to match the voter's preferences. We need now only define the ideal level of agreement among constituents about a representative to be unanimity—the natural assumption—and pure FPR follows. Other systems can be derived from different assumptions.[11] The Borda rule and ordinal FPR follow from ordinal ballots and a focus on minimizing the number of preference switches required from everyone in a constituency to have the same first choice. Different ideals (e.g., majority rather than unanimous agreement) can also lead to different institutional recommendations.

The Epistemic Interpretation of Democracy

The *epistemic interpretation* of democracy is based on the assumption that democracy is an instrument for the revelation of the common good. This idea is commonly attributed to Condorcet (1784, 1972) and Rousseau (1973), although the basic idea can be traced to Aristotle.[12] It has also been the subject of more recent debate (Cohen 1986; Coleman and Ferejohn 1986; Grofman and Feld 1988; Pateman 1986).

The fundamental assumption is one that Condorcet pursued early in his voting theory work: voters are judges attempting to evaluate some underlying reality (McLean and Urken 1994; Young 1988). Here, democracy is valued not for the satisfaction of wants or the protection of rights but for its ability to produce that which is collectively "good." Under this interpretation, the task of a voting system is to determine which alternative has the maximum likelihood of being the objective best, given the expressed judgments of individuals who disagree. A voting system is, in other words, nothing but a statistical device. "Misrepresentation" reflects not disutility or coercion but the voter-judge's assessment of the probability that a candidate is the wrong one.

With ordinal ballots, for instance, the Borda rule can be shown to be a maximum likelihood system under simple assumptions about the probability of individual errors of judgment. If we assume either (1) that each voter has some probability $p < ½$ of ranking two candidates incorrectly or (2) that the inherent "goodness" of each candidate affects his or her probability of being preferred to any other candidate proportionally, then the Borda rule chooses the maximum likelihood candidate.[13] The more fundamental rule with the cardinal rankings by voters is similar, although it requires us to ask voters somewhat strange questions such as, When comparing candidate A and B, what proportion of the time would you prefer A to B? In the multiple-winner context, FPR systems emerge when we assume that there is some ideal assembly (or *m* ideal underlying probability distributions) that the votes are attempting to reveal. Other voting systems reflect different models of individual and collective judgments.

Justifying Fully Proportional Representation

The point of this exercise is to show that the justifications for FPR as an ideal are not linked to one portion of the ideological spectrum. Indeed, many of the philosophical debates relevant to institutional design in democracies need not be resolved at all in order to recommend FPR. We have three completely different interpretations of democracy and the voting process. All of them suggest the same underlying goal. When a decision is to be made, the goal is to locate the alternative that would be the misrepresentation minimizer, as defined in the particular interpretation, if votes were sincere. When *m* representatives are to be elected, the goal is to find the *m* alternatives that would minimize misrepresentation in *m* equally sized voter coalitions. This goal recognizes the value of all preference information, as well as the value of letting voters segment themselves. At a minimum it would seem we have plausible justification for the more detailed examination of FPR systems.

MODIFIED FORMS OF FULLY PROPORTIONAL REPRESENTATION

I turn to the pragmatic concerns associated with implementing an FPR system. Pure FPR is a system that is not, cannot, and should not be implemented in practice. It serves only as an ideal against which we can evaluate the representation offered by alternative electoral systems. Pure FPR has two important weaknesses. First, while *m* was assumed to be relatively small in the example, most national assemblies have hundreds of seats. Assuming there are a number of candidates for each seat, voters could be faced with the prospect of assigning misrepresentation values to hundreds, or even thousands, of possible candidates. Ordinal ballots provide only minimal relief. I shall discuss how geography or other similar characteristics are (or can be) used to provide approximations, both good and bad, of ideal FPR results.

Second and more important, both the pure FPR scheme and the ordinal FPR systems are highly manipulable. All voting systems are manipulable (Gibbard 1973; Satterthwaite 1975), but not equally so. In the single-winner context, the Borda rule (ordinal FPR with $m = 1$) is perhaps the worst voting system when placed in the hand of strategic voters. Not only is the system very easy to manipulate—very little information is required (Nurmi 1987)—but it provides incentives for "outward manipulation" (Monroe 1993). Under the Borda rule, voters are encouraged to assess the preferences of other voters and report the exact opposite preferences except for one or two choices at the top. Voters are encouraged to express extreme opinions. The same is true under ordinal and pure FPR. There are, however, a few modifications to the system that can be used to determine "second best" alternatives in the face of strategic voters. I shall discuss three possibilities:

first-preference balloting, majoritarian choice, and approval balloting.

Geographic Districts and Other Fixed Segments

Nearly all national assemblies are elected using geographic districts. Israel's Knesset is the only pure exception.[14] Such districts can be characterized as a way to simplify elections and approximate representation results that would be obtained if the nation were a single election district (and voters could keep track of thousands of candidates).

Recall from the example that FPR provides voters with the opportunity to segment themselves. That is, voters are grouped together and assigned to a single representative when their votes indicate some common characteristics and when their group is large enough. If there are identifiable characteristics by which voters will group themselves anyway, it is a tremendous simplification to fix those divisions before the election. Geographically districted electoral systems can then be characterized as embodying an assumption that geographical characteristics are (or are highly correlated with) important dimensions along which voters would segment themselves if given the chance. Cementing such segmentations relatively permanently allows voters to concentrate on finer distinctions among a smaller number of candidates.

This assumption is a very slight one when district magnitude (m) is large, since many "minorities" can still establish a claim for representation. When m becomes small, as with single-member districts, the assumption is much stronger unless there are many districts (*assembly size* is large) or districts are drawn with nongeographic characteristics in mind. The 650-member British House of Commons provides an example in which large assembly size partially offsets the rough approximations inherent in single-member districts; the geographically dispersed Liberal Democrat voters are nevertheless still "underrepresented" by most measures. Following the Voting Rights Act, many American states have found it necessary to use nongeographic characteristics of the electorate to ensure appropriate representation of racial minorities within the system of single-member congressional districts.

This discussion applies to any possible characteristic along which electorates might divide themselves: race, religion, age, gender, candidate preferences, policy preference, or even time. One might argue that the purest FPR system would require an assembly with $m = n$ updated continuously as the electorate changes its membership or changes its preferences—a pure direct democracy. It is broadly accepted that we must approximate by electing a finite number of representatives, usually at intervals a few years apart. Most nations further assume that permanent or occasionally updated geographic segmentation will embody acceptable approximations, although the details vary a great deal. Similarly, it seems unlikely that an FPR system based on more than first-preference ballots would be possible in anything other than districts of moderate magnitude (although these districts need not necessarily be geographically based).

First-Preference Balloting

Once we have reduced m to a manageable size, the most obvious further modification of the pure FPR scheme is the one with which we are most familiar: first-preference balloting. Indeed, all forms of PR currently in use for national elections can be characterized as fully proportional representation schemes, modified to emphasize first preferences only. When voters express only first preferences, we have no information for assigning them to representatives other than those first preferences. In general, the optimal set of representatives is then the set of m candidates with the most first preference votes, although we have a number of different interpretations of this goal.

The conventional party-list PR system is probably the most familiar to the reader. Voters cast a vote for a single party and seats are awarded to parties in proportion to those votes. There are slight differences in how "proportionality" is interpreted in different systems that provide slightly different results, generally to the advantage of the largest parties (Balinski and Young 1982; Monroe 1994a). One particular system—the "Ste-Laguë" or "Webster" system—has as its goal the equalization of n/m (in our terms). If we take the pure FPR system and limit voters to expressing first preferences alone, we will get exactly the same results.

Similarly, the equivalent candidate-based conventional PR system is the *single nontransferable vote* (SNTV), used in Taiwan, Jordan, and, at least through 1993, in Japan. It has also been occasionally used (under different names) at a local level in the United States and the United Kingdom. Here each voter expresses a first preference, and the m candidates with the most votes are selected. Again, this is exactly the result that would be obtained under FPR if voters were limited to first preferences and the alternatives were individual candidates.

Cumulative voting, discussed earlier, is virtually identical to SNTV. Each voter is given m votes which may be distributed across m candidates or all given to only one candidate. Because voters are almost always better off (at least instrumentally) when they give all votes to a single candidate, cumulative voting is nearly the strategic equivalent of SNTV (Dodgson 1884a; Felsenthal 1985; Sawyer and MacRae 1962). So cumulative voting is a modification of pure FPR in which each voter gets to act like m voters, each of whom is limited to an expression of first preference (and each of whom we expect in most cases to act alike). As an aside, we can note that SNTV and cumulative voting have several undesirable characteristics relative to pure or ordinal FPR. It can be difficult for parties to determine exactly how many candidates to run and how to get voters to spread themselves optimally across candidates, a particular problem for large parties in large districts (Cox and Niou 1994). A special instance of this problem, also shared by simple plurality voting, is the possibility that votes will be split by similar candidates. For instance, some U.S. congressional districts drawn to provide minority representation have failed to do so, because several minority candidates ran and split the minority vote. Pure FPR is not subject to these problems.

Open-list PR, as used in Finland, nests an SNTV-like mechanism inside a closed-list PR mechanism. Votes are cast for single candidates, each of whom is attached to a party label. Seats are first distributed to parties as under closed-list PR (pure FPR modified to limit voters to expressions of favorite party) and are then distributed within parties to individual candidates as under SNTV (pure FPR modified to limit voters to expressions of favorite candidate).

Some extant electoral systems allow voters to express more complicated preferences but still place emphasis on first preferences. The best known is the *single transferable vote* (STV), used in Ireland and Malta, and advocated by some reformers for use in Britain. We can characterize STV as a variant of the ordinal FPR system in which only a limited number of the possible representative sets are examined. Voters are assigned to their first preference representative. If there are too many or too few segments formed, voters must be shifted to other representatives. As with FPR, some popular representatives may have more votes than they need.[15] To avoid dilution or wasting of their vote, some of these voters can be transferred to other representatives. Each excess voter is transferred to the next-highest-ranking alternative on the voter's list (that is still viable and needs votes). Once excess voters have been shifted, there may still be too many segments. The candidate with the smallest segment of voters is then eliminated and these voters are shifted, again in each case to the highest-ranking candidate that is still viable and needs votes. This continues until there are only m segments. So segments of voters are formed on the basis of first preferences, and lower preferences are used only to shift voters. Single transferable vote can then be characterized as a modification of FPR that segments voters with representatives in a way that minimizes total misrepresentation but that only examines a small number of possible representative sets. Note that the method for calculating the FPR solution that I have suggested involves examining *every possible representative set* and, as a computational shortcut, initially assigning each voter to the candidate of highest preference *within the set under consideration*. For example, if $m = 1$ and there are three candidates {A, B, C}, we check each possible representative set, {A}, {B}, and {C} for total misrepresentation. This is quite different from STV, which assigns voters to their overall first preference and moves them to other candidates only if their first preference has been elected or eliminated. In the example, if {B} is second on every list, {B} might be elected under FPR but not under STV. This is what makes STV "first-preference-based" and FPR not. We would then expect STV to be an inefficient approximation of ideal FPR results, although perhaps better than SNTV or list PR.

First-preference-based systems have three possible advantages over FPR systems (again, I should emphasize, as systems of *representation*). First, they are simple to understand. One might argue that first-preference balloting is simply a second-best procedure for approximating ideal results with a more understandable system. Second, voters may hold only first preferences: the idea of party identification would imply such behavior. Third, these systems can be motivated by the same interpretations of democracy, although in each case by arguably inferior assumptions. For instance, under the utilitarian interpretation, first-preference systems can be seen as a system of "maximax" utility, whereas pure FPR maximizes the sum of individual voter utilities. Under the minimum coercion interpretation, we might say that first-preference systems seek to minimize the number of voters forced to be represented by someone other than their first-choice candidate.

The disadvantages of first-preference voting are more numerous and more important. First, voters do seem to be able, when asked, to express preferences that are at least to some extent more complex. If more complex preferences do exist, it is surely the task of a representation scheme to represent those complexities if possible. Like pure FPR, first-preference systems also have undesirable manipulation characteristics (Monroe 1993). The approximation to fully proportional representation is likely to be very poor under almost any combination of sincere or strategic voting and, in nearly all cases, inferior to those of the approval or majoritarian systems to be described. Finally, the assumptions underlying first-preference-based voting are, I believe, less justifiable (e.g., from behind a Rawlsian veil of ignorance) than those underlying FPR. Ultimately, therefore, it seems that while first-preference-based PR systems hold some intuitive appeal, they are likely to solve few of the important problems associated with pure FPR and introduce new pathologies that are probably best avoided. These pathologies will be particularly strong in electoral systems where the number of constituents per representative (n/m) is large. Although not the case in the newspaper example, this is nearly always the case for modern national assemblies (e.g., $n/m \approx 75{,}000$ in the United Kingdom, 500,000 in the United States).

Majoritarian Choice

A common solution to the manipulability of the Borda rule in the single winner context is a shift to less manipulable majoritarian decision rules. Black did this in his trial scheme, for instance, by basing representation on segment medians (a majoritarian principle), not means (a Borda-like principle). A similar principle might be applied to find a less manipulable version of FPR.

Black's median procedure works only for one-dimensional single-peaked preferences, however. Extending the majoritarian concept to more general sets of preferences is far from trivial even in the simpler case of selecting only one winner. This is ultimately the problem underlying Condorcet's voting paradox and Arrow's General Possibility Theorem. I have elsewhere argued that the best solution to this problem is a system I have labeled the *voice of reason* (Monroe 1993). For any set of alternatives, we can look at each pair and determine which of the pair is preferred by a majority. The voice of reason finds the single voter who agrees with the most majorities and selects that voter's first preference.[16] In more intuitive language, the system finds the single most "moderate" voter and selects his or her first choice. The incentives under this system are for voters to express top preferences sincerely (in case they are the voice of reason), to express intense preferences sincerely (in order to influence who becomes the voice of reason), and to agree with as many other voters as possible for less intense preferences (in order to be the voice of reason).

This system can be extended to an FPR context, but I have not yet found a simple way to do so. The complex way requires that we use a form of cluster analysis (implicit in Black's trial scheme) to first cluster voters into equally sized and relatively homogeneous segments. We can measure the homogeneity of any segment of voters by the number of preference switches (e.g., changing one voter's preference ranking of $x_1 > x_2 > x_3$ to $x_2 > x_1 > x_3$) that would be required for all voters in the segment to agree unanimously across all pairs of alternatives (Kemeny 1962, 9–23). Once we find the optimal clustering of voters, the voice-of-reason winners for each segment are selected.

The primary difficulties with this system are practical. First, the system would be very difficult to explain to voters. Second, the system is nearly impossible to implement, because it involves an optimization across all possible segmentations of the electorate, not just all possible sets of representatives. Indeed, for realistically sized electorates, such an optimization is beyond available computational capabilities. Very good approximations can be found through techniques such as genetic algorithms, but there can be no guarantee that the "true" result has been found. Third, it is not clear that this system has the manipulability characteristics that motivated it. Indeed, the homogeneity measure is itself, like the Borda count, a measure that would be manipulable. We could again resort to a majoritarian homogeneity measure, but this introduces additional computational complexity. Although I am unable to offer a feasible majoritarian FPR system at this time, it still seems likely that, with simplifications such a system might prove to be a viable alternative.

Approval Balloting

The third and most promising option requires a change in FPR ballots. Under approval balloting, electors vote for as many candidates as they like, "approving" of those for whom they vote and "disapproving" of others. For $m = 1$, this is simply the well-known approval voting system (Brams and Fishburn 1983). For $m > 1$, the system operates as with the full lists, only more simply. Under approval balloting a voter is assumed to experience no misrepresentation when segmented with an approved representative and one unit of misrepresentation when segmented with an unapproved representative.

Approval-based FPR has four practical advantages over ordinal FPR: (1) approval balloting is easier for voters to understand and implement than complete preference lists; (2) optimizing with approval ballots is computationally easier than in the pure system with full lists; (3) although approval balloting forfeits some of the information that can be provided with full lists, flexibility in the number of approved candidates allows it to capture some aspects of preference intensity that full lists cannot, so that while something like first-preference balloting is clearly a more crude approximation of pure cardinal preferences than ordinal balloting, approval balloting is not; and most important (4) approval-based FPR is manipulable in much more normatively desirable ways than pure FPR.

I have shown elsewhere that under approval voting, "extreme" voters (voters who, in the terms of this article, have values for μ_{ia} that are quite unlike the group averages) have incentives to increase the number of alternatives for whom they approve (Monroe 1993). They have an incentive to compromise and become more moderate in their expressed preferences. "Moderate" voters (voters whose values of μ_{ia} are close to group averages) have an incentive to decrease the number of approved alternatives. In sum, approval voting creates extreme moderates and moderate extremists. The result is a greater likelihood of selecting a moderate outcome. In the face of strategic voters, the "sincere" Borda winner (the "real" utility maximizer) is more likely to be chosen under approval voting than under the Borda count. This effect is particularly pronounced in realistic election situations where polls provide the voters with dynamic information about the preferences of the rest of the electorate and the collective effects of votes. Losing voters expand their set to have *some* influence; winning voters reduce their set to have *more* influence. The same dynamic will be observed under the approval-based FPR scheme, except that "moderates" and "extremists" are defined relative to the segment containing them.[17]

We can illustrate approval-based FPR with the newspaper example and $m = 3$. For the sake of illustration, assume that voters with incomplete lists approved of those they listed and that those with complete lists approved only of their favorite newspaper (see Table 1). These assumptions are undoubtedly unrealistic but serve the purposes of illustration. The winning set is {the *Financial Times*, the *Times*, the *Independent*}. Only voters 22, 26, and 28 cannot be assigned to an approved newspaper. All other sets of newspapers must leave more voters unrepresented.

Although illustrating the operation of the system, the example may be more misleading than helpful, because the dynamics of manipulation could not be observed (nor could they be in the ordinal FPR case). Such observations would have to be made under experimental conditions. Since it is the contrast between the dynamic manipulation characteristics of the two systems that provides the strongest argument for approval-based FPR, such experiments would be a valuable step in the development of practical FPR schemes.

Because approval-based FPR (call it *approval representation*) is the system I most strongly recommend for consideration as a serious alternative by electoral reformers, let me state clearly how such a system would be implemented in practice. Voters vote in districts of magnitude *m*. Candidates are listed on the ballot. Voters are asked to "place a mark by all candidates that you would approve as your representative in the assembly." When voting is complete, tellers examine each possible set of winners to determine the set's "misrepresentation." This is done by assigning each of the *n* voters to one of the *m* candidates in the set under consideration, being sure that no candidate is assigned more than *n*/*m* voters. (The teller's task may be completed manually by piling the ballots and moving them among candidates, as in Irish STV elections, or more quickly by computer.) The voters are transferred among candidates until as many voters as possible are assigned to a candidate for whom they voted. The number of voters assigned to candidates for whom they did not vote is that set's "misrepresentation." The candidates with the lowest "misrepresentation" value, as a set, are declared the winners.

THEORETICAL ALTERNATIVES TO FULLY PROPORTIONAL REPRESENTATION

Fully proportional representation is not the only concept of how full preferences can be reflected in a proportional representation system. I shall compare FPR with the three most important theoretical alternatives: the *committee selection function*, *preference-ratio PR*, and *assembly-based PR*.

The Committee Selection Function

Chamberlin and Courant (1983) express a strong concern over the lack of attention given to the application of social choice theory to the theory of representation, a concern that I share. Their recommended system, the committee selection function, is very similar to the ordinal FPR scheme outlined here. They also argue for the committee selection function from philosophical grounds (a subset of those used here) and note its tight relationship to the Borda count. There are two important substantive differences, however, that make the committee selection function unworkable: unequal constituencies and the use of weighted majority voting.

The committee selection function is essentially the same as the ordinal FPR scheme outlined here except that voters are segmented into constituencies without concern for the equality of those constituencies. Instead, each voter is assigned to the representative that she ranks highest of those elected. This avoids the "problem" of assigning some voters to less-preferred representatives but will not generally provide constituencies of equal size. Chamberlin and Courant suggest then that each representative be given legislative votes in proportion to their constituency size. The problems with weighted voting and with interpreting the "power" of differently endowed voters in such systems are well known (e.g., Banzhaf 1965). Chamberlin and Courant discuss a few of these problems but do not quite come to grips with the now widely accepted assertion in Banzhaf's title, namely, "Weighted voting doesn't work."

Consider again the newspaper example in Table 1, making the leap of faith that these are potential legislative candidates, not newspapers. Assume we are examining the potential representative set {F, I, T}. The committee selection function assigns each voter to the candidate ranked highest of those in the set: 10 voters to F, 15 to I, and 8 to T. The inequality of constituency size is "corrected" by giving 10 votes to F, 15 votes to I, and 8 votes to T, in the assembly where decisions are made by majority rule (17 votes). It is easy to set that this difference in weights is irrelevant: any two can outvote the third. We can just as easily find examples where representatives have votes that are meaningless. There is no solution to such problems. Again, the appropriate logic for "excess voters" is to provide them with the opportunity to influence the selection of additional representatives. This is the logic used in FPR.

Preference-Ratio Proportional Representation

The discussion herein of FPR has three parallels with the discussions of Rapoport, Felsenthal, and Maoz (1988a; 1988b): (1) they offer normative ideals ("desiderata") against which representation schemes can be evaluated, as well as a number of schemes for closely approximating these ideals; (2) their schemes are motivated by a desire to understand what "proportionality" means when preferences beyond a favorite are expressed; (3) they begin their discussions by quoting Duncan Black's thoughts on the subject of PR. Unfortunately, however, their citations to Black are limited to his well-known book (Black 1958) and do not include reference to his earlier work on the subject (Black 1949). In this earlier work, Black discusses a scheme identical in concept to that discussed by Rapoport, Felsenthal, and Maoz but also notes that it has "one fatal flaw" that would prevent its use in practical situations. The flaw is that such schemes provide parties with the incentive to split into smaller and smaller parties. Black does not discuss these schemes in his book, presumably because of this flaw. The flaw is also a feature of the Rapoport-Felsenthal-Maoz schemes.

The concept of proportionality embodied in the Rapoport-Felsenthal-Maoz representation systems—*preference-ratio PR*—is that the ratio of "seats awarded to part A" to "seats awarded to party B" should, as nearly as possible, match the ratio of "voters who express a preference for party A" to "voters who express a preference for party B." As with conventional PR, there are many subtleties of interpretation that can affect the exact method chosen; Rapoport, Felsenthal, and Maoz discuss a number of alternatives.

No system can guarantee that the preference-ratio criterion will always be met perfectly, because collective preferences rarely allow it. Consider the conventional voting paradox, in which voter 1 prefers party A to party B to party C (v_1: A > B > C) and, similarly, (v_2: B > C > A) and (v_3: C > A > B). The preference-ratio criterion would suggest that the ratio of seats given to party A, s_a, to seats given to party B, s_b, should be 2:1, because there are two voters (v_1 and v_3) who prefer party A to party B. Similarly, however, the criterion suggests that the ideal distribution also be such that s_b:s_c = 2:1 and s_c:s_a = 2:1. There is no way to distribute any number of seats so that A gets twice as many as B, B gets twice as many as C, and C gets twice as many as A. The need for approximation need not bother us, however. In this example, all of the Rapoport-Felsenthal-Maoz schemes would give each party an equal number of seats, an outcome that corresponds to our intuition.

The "fatal flaw" is rather the incentive given to parties to clone themselves. Consider a simple example in which there are two parties, A and B, and voters are split evenly on preferences between A and B. Preference-ratio PR gives each party half of the seats, as we would expect. Now assume that party B splits itself up into two identical parties, B_1 and B_2. For the sake of simplicity, assume further that since B_1 and B_2 are identical, voters are split evenly on preferences between B_1 and B_2. There is then a quarter of the electorate with each of the following orderings: A > B_1 > B_2, A > B_2 > B_1, B_1 > B_2 > A, and B_2 > B_1 > A. The preference ratio for each party pair is 1:1, so preference-ratio PR distributes one-third of the seats to each of the parties. Party B has raised its seat percentage from 50% to 67%, simply by cloning itself!

Fully proportional representation schemes do not have this property. Ordinal FPR results in this example would distribute 50% of the seats to party A, 25% to party B_1, and 25% to party B_2. Fully proportional representation systems (pure or otherwise) provide no advantage from cloning or other types of factionalization.

Assembly-based Systems

Assembly-based representation systems offer a more fundamental challenge to conventional theories of representation than do FPR systems. Brams and Fishburn (1993) and Benoît and Kornhauser (1994) point out that although all extant electoral systems ask voters to express preferences over parties or candidates, it is assemblies (and, indirectly, public policies) that they are choosing and over which they do (or should) have preferences. Fully proportional representation and assembly-based representation are incompatible concepts and cannot be reconciled. I offer a few arguments here for my belief that FPR is a more defensible concept, but this remains a question for further discussion.

Brams and Fishburn illustrate that if voters do hold such assembly-based preferences, candidate- or party-based electoral systems will generally do a very poor job of reflecting those preferences. Brams and Fishburn offer a practical scheme for assembly-based representation, namely, *yes-no voting*—essentially, approval voting over possible sets of representatives. Assembly-based representation essentially converts the multiple-winner social choice problem defined here back into the original single-winner problem. As such, the same problems must be faced, with the additional difficulty that even a small number of candidates can be combined into a great number of possible assemblies. Even social choice theorists must be daunted by the possibility of a ballot with hundreds or thousands of alternatives. Given these difficulties, *yes-no* voting is probably the most defensible assembly-based system. Just like approval voting, it is a relatively simple system that will provide good approximations of normatively desirable results, regardless of the level of voter sophistication.

I would argue, however, that assembly-based systems circumvent the goals of representative government as embodied in the use of multiple-winner assemblies. Consider the task of the elector who faces the ballot-of-a-thousand-assemblies envisioned above. There seem to be only two types of votes that might be cast. The first would be a vote for an assembly consisting solely of representatives from one party. It seems likely that many such votes would be cast, and the resulting assembly would be highly homogeneous (even if, because yes-no voting were being used, the dominant party was a relatively moderate or "representative" one). Some electors, on the other hand, might vote for an assembly that would provide approximately the same outcomes as a single-party assembly but with some recognition that more diversity was needed. As with cumulative voting, however, there seems to be little or no incentive for electors actually to cast such votes, so we can probably expect such behavior to be limited. It is my expectation, then, that assembly-based electoral systems would be likely to elect one-party (or at least dominant-party) assemblies. If this is the case, it is not clear why an assembly as such is needed: a single elected official could accomplish the same task.

In fairness, this argument can be turned on its head and directed at FPR. Recall that I assume, in deriving FPR systems, that in a multiple-winner setting, the only "misrepresentation" of significance is that in the connection between a single voter and his or her representative. In other words, it is assumed that voters do not have preferences over possible assemblies but only over possible individual representatives. If voters do have assembly-based preferences, what is to keep them from manipulating FPR systems accordingly? Might a moderate voter be given the incentive, under an FPR system, to vote for a extreme right-wing candidate in order to "offset" the extreme left-wing candidate that others may elect?

Indeed, such incentives will be present, but as the system approximates the ideals of FPR—as the assembly grows large, as the number of predetermined segmentations of the electorate grows smaller, and so on—these incentives will be smaller and apply to fewer voters. In the limiting case of an infinite assembly, the optimal representative for any particular voter is just like that voter. It is only in very small assemblies, like the two-person assembly assumed in the question, that such incentives would have significant impact. Nevertheless, there is a fundamental philosophical difference between assembly-based PR and FPR that deserves further attention.

CONCLUSION

There are a number of further research steps, theoretical and applied, that might be taken to broaden our understanding of fully proportional representation. On the theoretical front, we might pursue an axiomatic characterization of FPR (along the lines of Myerson 1993) or a prediction of candidate or voter equilibria under particular FPR systems (along the lines of Cox 1987, 1990). Fully proportional representation no doubt has some interesting properties (e.g., amonotonicities and clone independence) that need to be identified and whose importance needs to be determined. Tests of the system under experimental conditions seem likely to provide insight into the relative usefulness of the different types of FPR that have been described here and more conventional electoral systems. To be useful, these experiments must reflect realistic political conditions, with a variety of voter/party combinations and with dynamic information from polls. We must also be concerned with the national effects of implementing a district-based system of FPR and the effects FPR would have on the development of party organizations and party systems.

I am not advocating the adoption of FPR in actual legislative elections. There is still far too much to be learned about the system. I cannot stress enough that I have assumed throughout that "representativeness" (however defined) is the sole objective of a multiple-winner electoral system. Before advocating FPR, this of course must be weighed against other objectives, such as the encouragement of a responsible party system, and an effective government. Indeed, many of the arguments in the ongoing "plurality versus proportional representation" debate would still be valid in a critical examination of FPR. As with any institutional design, we must be concerned that the institution fits the particular needs of the situation. While FPR seems a good system for newspaper selection, uncertain links with policy formation should give us pause as we consider FPR for adoption in, say, city council, congressional, or parliamentary elections. I cannot resolve such issues here but would assert that perhaps the debate should shift its focus from "plurality versus PR" to "assembly-based representation versus FPR."

I do advocate further investigation of the concept of FPR and the schemes for its implementation. Approval representation seems particularly attractive as an object of further institutional analysis. Fully proportional representation appears to have properties identified by other authors as desirable in their defenses of alternative systems, and it is the system that most closely fits several normative ideas about what it is that an electoral system ought to do. Until other systems are shown to be uniquely better than FPR, it merits inclusion in any further discussion of alternative electoral systems.

Notes

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1. I shall discuss why I consider a system like the single-transferable vote, which asks voters to express more than one preference, still to be "first-preference-based." There have been some discussions of completely different types of representation system in which voters express preferences over sets of alternatives rather than individual alternatives (Benoît and Kornhauser 1994; Brams and Fishburn 1993). I shall take these up shortly.

2. This idea does not appear at all in his main work on PR, *The Principles of Parliamentary Representation* (Dodgson 1884a; see also McLean, McMillan, and Monroe 1995).

3. Under block voting, voters cast ballots for m candidates. The m candidates with the most votes win.

4. The conventional implementation of the Borda rule/Borda count has voters list the c candidates in order of preference. The first candidate is awarded $(c - 1)$ points, the second $(c - 2)$ points, . . . , the last 0 points. The candidate with the highest Borda score is elected. Equivalently, the Borda winner is the candidate who receives the most votes in straight pairwise contests with every other alternative (Borda 1784).

5. If we want to achieve exact equality when n/m is not an integer, voters can be placed fractionally in two or more coalitions. The practical significance of this issue is minimal in a mass electorate, so a discussion of the procedural details is not given here.

6. There is a more subtle interpretation of such ballots as abstentions (i.e., as expressions of deference, rather than as expressions of indifference, which has slightly preferable

properties, but the exposition of this interpretation would require a lengthy digression from the central argument.

7. Because no voting system was specified, argument were made for several different ways of counting the ballots (with different results, of course). In the end, the voting was voided, and decisions were made under an amendment procedure.

8. Under plurality rule, voters cast votes for one candidate. The candidate with the most votes wins.

9. Under approval voting, voters cast votes for as many candidates as they like. The candidate with the most votes wins.

10. Another version of the utilitarian interpretation, the *voter-averaging interpretation* of democracy, is summarized succinctly by Black: "That candidate ought to be elected who, on the whole or on the average, stands highest on the electors' schedules of preferences." (1958, 55). In statistics, the "average" of a distribution is a single number that best represents the distribution as a whole. In voting, the goal is to select an alternative or social utility function that alone best represents the distribution of voter utilities. The Borda rule, for instance, is the voter averaging system that finds the mean voter preference ordering. Other voting systems can be based on averages of other representations of voters preferences or other concepts of "average" (e.g., median or mode). In the FPR context, we seek to find m candidates that best represent the average of voter preferences in m equally sized segments. Black's "trial scheme" is such a system, where the median is the averaging concept.

11. Note that there is also an implicit assumption of candidate equality here as well. If we assume, in the manner of Buchanan and Tullock (1962) for instance, that coercion is relative to the status quo, then our institutional recommendations under this interpretation will be different. We might instead view a system that maintained incumbent representatives until unanimously defeated as the minimum coercion system. This is superficially undemocratic enough in this context that I do not consider it in further detail here.

12. "For it is possible that the many, no one of whom taken singly is a sound man, may yet, taken all together, be better than the few, not individually, but collectively" (Aristotle, *Politics* 3.11.1281a39).

13. The latter is, in fact, a recasting of the conventional Bradley-Terry psychometric method for inferring from individual judgments which of a set of objects is best (Bradley and Terry 1952).

14. The Netherlands also uses a single nationwide "district" to determine the distribution of seats to parties but does consider geography in the distribution of seats to specific candidates.

15. The quota here is actually not n/m but the smallest integer greater than $n/(m + 1)$ (the *Droop quota*). This is the smallest number that ensures that no more than m candidates will be elected. The originator of the scheme (Thomas Hare) advocated n/m as a quota, but STV with this quota can elect fewer than m representatives.

16. If there is a tie among two or more voters, then a Borda rule among these voters determines the winner.

17. This is a intentionally vague statement. Exact equilibrium predictions depend on assumptions about voter preferences and information. Where voters care only about their individual representative, the system is strongly robust to manipulation. When they care about the consequences of assembly choice as a whole, we have no equilibrium prediction for this or any other electoral system (see Austen-Smith and Banks 1988 for a discussion of the problem here). Intuition, however, suggests that approval representation will do at least as well as alternatives.

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THEORIZING DIFFERENCE: VOICES FROM THE MARGINS

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Theorists of widely different persuasions are making serious efforts to understand difference and to bring the concerns and ideals of others within the reach of intellectual and political visions. Yet this attention to difference continues to be examined almost entirely through precise, rigorous, and sophisticated theoretical language. This is only appropriate if we presume that this language, the language-of-theory, is able to represent—and serve as the master interpreter of—all ideas and ideals, all senses of self and politics. I believe that this presumption is deeply flawed. Theoretical language passes over and distorts the differences it would understand. Theory more attentive to difference needs to gain access to the meanings that circulate within different lives, especially as reflected in literary writing of those who, themselves, speak and write from sites of difference.

> The footprint in the sand that so worried Crusoe's nights, that compelled him to build a fortress, and then another to protect his new world order, disappears from his nightmares once Friday embraces, then internalizes, his master's voice and can follow the master's agenda with passion.
>
> —Toni Morrison, Introduction to *Race-ing Justice, En-gender-ing Power*

> What I resent most, however, is not his inheritance of a power he so often disclaims, disengaging himself from a system he carries with him, but his ear, eye, and pen, which record in his language while pretending to speak through mine, on my behalf.
>
> —Trinh Minh-ha, *Woman, Native, Other*

> For the master's tools will never dismantle the master's house.
>
> —Audre Lorde, *Sister Outsider*

There is now little doubt that our sense of who we are and what we hold to be true and good stops at the water's edge of a common language and the shared meanings about personal or social reality that are produced by and carried on within that language. In the face of voices that insist on difference, there are growing doubts about the reach of any cultural or political common-ness. An older presumption that one common language is able to represent the tremendous array of gender, ethnic, and cultural differences based on its capacity to serve as a neutral, consensual passageway seems impossible to maintain.

Yet the problem of "What is to be done?" theoretically and politically in this time of difference is far from being resolved. Mainstream theorists (read liberal and communitarian)[1] adopt a view of difference-as-sameness, presuming that underneath all expressions of gender or ethnic or other differences, a reasoned and informed reading of "our" political tradition shows that on fundamental matters "we" all do agree or through dialogue can reach agreement (Flathman 1987, 1992; Rawls 1987, 1993; Taylor 1992). By contrast, poststructuralist- or postmodern-inspired deconstructive theorists keep insisting on a kaleidoscope of ungrounded differences, a grab-bag of contingent, genealogically shaped desires and prejudices and tales masquerading as reason and truth (Connolly 1993; Derrida 1981a, 1981b, 1982; Foucault 1979). With perhaps greater intellectual and political urgency but also working irreverently within and outside the canon's long shadow, a rich if long-silenced mosaic of dissenting voices by feminists and scholars of color have denied theoretical claims of a single, common intellectual and political tradition (Allen 1986; Anzaldúa 1987; Butler 1993; Cixous 1976; Cliff 1980; de Lauretis 1987, 1990; Haraway 1991; hooks 1989; Irigaray 1985a, 1985b, 1993; Minh-ha 1989; Spivak 1987, 1993). Speaking and writing for and often as those who are Other, these theorists insist that there are distinct and perhaps deeply opposed differences—sometimes shaped through dominant notions of biology or race or culture; sometimes adopted for reasons of ethical necessity; sometimes lived as joyful, creative, mixed identities outside the reach of domination—that undermine presumptions of a shared political tradition and its esteemed, central values. In recent decades, theories that have been put forward from each of these varied sites now dot the intellectual landscape.

The implications of these disparate theoretical approaches for the ways in which we try to understand difference—how we view the tasks of theory, the language(s) we use to carry on and present our views—have received rather little attention. The problem is viewed as a matter of picking just the right theory and getting on with the work of analysis, not one of questioning the language with which theorizing is carried on. Yet I doubt that conventional theoretical language and analysis as framed by that language are (still) able *to pass* as the intellectual cart large enough to carry all differences within it. With respect to theorizing difference, the language-of-theory remains . . . undertheorized.

A number of ideas and practices associated with *passing* will be used to help diagnose what I view as unavoidable flaws when rigorous theoretical language is used to detect the pulse of difference in its changing rhythms and varied locations. The problematics of difference within the arena of political theory are related to issues of passing and passages in a number of ways. Those using theoretical language presume that this language can pass as the discursive voice of all, capable of fairly and fully representing who people are and would be and what binds or divides them—tracing the maps of their identities and intuitions, articulating their hopes and fears, divining the political ideas and arrangements by which they would live. On the basis of such presumptions, theorists seem to view their language as a (the?) neutral passageway through which all identities and differences can be seen and interpreted.

The practice of passing is well known to those who live in society's margins. Many who reside in the margins come to know, through what they are taught and through bitter experience, that a high price is paid for being named as Other. They spend their lives silently but (in)visibly shaping and measuring themselves by a dominance that so long as they successfully pass and their secret is kept, enables them, on occasion, to be let in. Still others refuse to try to pass even if they can—resist passing even if it seems they must. And in poorly understood ways, those who have felt compelled to pass or learned to pass or refused to pass may have developed ideas of autonomy and community and other political "concepts" that are neither heard nor seen by those who, with a nervous confidence, stick to the dominant passageways of social and political life.

From Mary Wollstonecraft (1975) and Virginia Woolf (1929, 1931), to Hélène Cixous (1991, 1993) and Audre Lorde (1982, 1984), to Michelle Cliff (1980, 1985, 1987) and Gloria Anzaldúa (1987), many women and minority writers have written volumes about these efforts, in countless sites and for a long time. Their challenges have called into question dominant philosophical ideas and visions about who they are and the circumstances within which they (would) live. For the most part, however, the philosophical voice's language-of-theory and the sources from which it is drawn remain untouched. Implicitly or explicitly, the language-of-theory is presumed to pass as the master linguistic voice that, without loss or distortion, is able to represent all the complex differences and histories embedded within them.

I suspect that an indispensable part of our being (somewhat) able to understand and comment on the lives of those who reside in society's margins is that we have some way of connecting, some way of imaginatively and reflectively passing over into parts of their lives, of reaching into where they have been and what they have become, of (re)tracing the paths that have led them there, of understanding what they know and how they know it, of feeling what they cherish and what they loathe. We need to reconsider the ways in which a dominant language (the language-of-theory?) presumes to speak for all. As we do so, we may begin to see the ways in which women, minorities, and others outside society's mainstream have developed resistances to that language and, in doing so, fashioned tongues of their own.

I want to explore such matters; in doing so, I shall consider how we might pass somewhat beyond the language-of-theory to carry out the tasks of theorizing in this time of difference. Partly for such reasons but also because it offers an instance of how it may be possible to extend theory's reach, I want to write briefly about some of my experience with passing. Hoping that I do not improperly appropriate the term, two events come to mind that reflect my own limited experience with passing—that help me to imagine, at least to some extent, the ways in which the demands of passing seduce and frighten and discipline.

The first incident involves my father. Though receiving less than an eighth-grade education, he was an active and respected member of a small farming community in central Wisconsin several decades ago. A sign of this esteem was that everyone called him by his given name, Gerald. And that was the name he always used for himself. He served on the local school board and township board, was active in a Masonic Lodge, and read widely on political issues in local and statewide newspapers. For reasons largely beyond his control (asthma and the declining fortunes of small farms), he moved, when he was nearly 50, to a large city in the Southwest. He looked in vain for full-time employment for over a year—selling brushes and detergents door to door, trying to sell real estate from a too-ordinary car, doing seasonal work on a large dairy operation. Finally, he came home one day and told us that he had a good, permanent job as a full-time janitor in an elementary school. To do so, he sensed strongly that he had to pass downward, as it were: at the school, he told everyone that his name was Jerry, and that was the name he and they used from then on.

An "advantage" for me of the long move to the booming Southwest was finishing my early education in a large, not-quite-suburban school rather than the one-room school of the early years. There was also the opportunity, almost certainly unavailable otherwise, of attending college. In the process, my barnyard language and vocabulary, generously spiced with long cursing and short, infrequent sentences, was slowly abandoned. I learned, in the words of a Don Williams song, to talk like the man on the six o'clock news. Along the way (but by then, I knew it was only a way-station),[2] I followed in my father's footsteps and worked full-time as a janitor in a large hospital. Holding a "float" position, I eventually worked on nearly every wing and floor of the hospital—mopping floors, cleaning toilets, emptying trash cans of discarded litter and cigarette butts. Unless they were asking me to do a particular task, I recall no occasion during the year or so of my employment when any professional person, doctor or nurse or technician, ever spoke to me.

This leads to a second experience of passing. A few years ago, well into my academic career, I had occasion to be near that same hospital about lunch time.

For reasons of convenience and familiarity, I decided to eat at the hospital's cafeteria. Something quite remarkable happened as I walked through the hospital in my standard academic uniform of grey slacks, navy-blue blazer, white shirt and tie. Almost every nurse and doctor made a point of speaking to me: "Good morning!" "Hello, how are you?" To make sure that I wasn't misreading the situation, I turned around and walked through several more floors and wings of the hospital; and the same thing happened, over and over. As a white, professionally dressed, mature-looking man, I passed as a medical doctor without even trying.

I use personal narrative to help introduce this essay with some uneasiness. It seems self-indulgent, it reflects personal nongeneralizable experience, it is incompatible with the task of theoretical analysis and reasoning. Yet it is an appropriate way of entering what I want to say about how we might re-vision[3] theory for purposes of better understanding difference and the role that marginalized voices may play in that project. In particular, the imaginative reach of personal narrative and other forms of more literary writing allows us important access into the lives of particular people in particular places. I am reminded of Nancy Miller's (1991, chap. 1) suggestion that a regular dose of the more personal reflections that are central to literary writing is a healthy antidote to the generalizations that characterize so much theorizing. And I am encouraged by Toni Morrison's simple observation that "narrative remains the best way to learn anything" (1993, 372).

Imaginative, literary writing need not be antitheoretical. Looking at ourselves and others with the more or less simultaneous helping hands of literary and analytical writing affords a larger passageway through which to gain a better sense of what we and others share or hold in common, what unites or divides us, what may be unavoidably, irretrievably different. Theory choreographed by literary writing may still have a place in its dances for carefully prescribed, analytical movements; yet literary and analytical languages need to learn how to dance together, leading and following and moving together in turn as suggested by the tempo of different rhythms. To see better that this is so requires some consideration of ideas that have surfaced with respect to the ways in which the language(s) we use and the senses we have of our selves and others are deeply interwoven.

PASSING FOR: THEORETICAL LIFE AND THE LANGUAGE-OF-THEORY

The view that language is central to how we understand ourselves and others is now rather unproblematic (Derrida 1982; Kolodny 1985; Miller 1988; Rorty 1989; Showalter 1982, 1985; Wittgenstein 1953; and countless others). I want to focus initially on a few ideas of Wittgenstein because he both supports much of what I want to suggest and represents a view of theoretical or philosophical language that I contest. Wittgenstein (1953) suggested that language has been developed among those who share a form of life. As we participate in a form of life, we learn the practices by which it is carried on: we learn to use its language. We become who we are and we learn to interpret ourselves and our circumstances by gaining linguistic mastery, by using the language we acquire to characterize the activities we engage in. We are and do what we speak; we speak what we do and are. More specifically, we acquire a facility in language by participating in language games. As explained by Wittgenstein, "Here the term 'language-*game*' is meant to bring into prominence the fact that the *speaking* of language is part of an activity, or of a form of life" (pp. 11–12, emphasis original.)

Wittgenstein's ideas, now widely accepted in philosophy and beyond, call into question essentialist conceptions of sameness or difference. People find it necessary to pass or to resist passing because they have acquired a language—its words, its meanings, the practices associated with those words and meanings—that tells them who they are, not because they are biologically or culturally or otherwise essentially the same or different. Women and gays and lesbians are constructed and named as such—as Other—through a language they adopt or resist. They are not born different, and they are not the produced and opposed subjects of materialist historical forces or the natural carriers of different, vested interests. Whether they conform or resist, they do so within a language or within linguistic innovations that circulate within or stretch and reform a dominant language. This means that views of difference that emphasize contrasting "ways of life" as though these were naturally or as an empirical matter attached to different sorts of people are misleading. Such "empirical" patterns emerge because they reflect the ways in which linguistic meanings and practices have played out within the context of social life. They reflect the sedimented results of lived and internalized linguistic codings, not the patterned outcomes of natural (sexual, racial) or historical (interest, class) forces. Skin color, certain organs, and the ownership or nonownership of something named property do not demand, ex nihilo, to be viewed as socially significant markers. Their selection as critical markers to be named as race, sex, and class and the filled-to-the-brim meanings attached to those names are intimately connected to a particular language and to the form of life within which that language is embedded.

I think this perhaps-obvious point is worth emphasizing, because it helps me both to focus my discussion of how to theorize difference in a time of difference and also to challenge the ways in which this theorizing has been done. It means, to follow Wittgenstein's lead, that it is no longer sensible to view different sociological "ways of life" as anything more than the distinct meanings and practices—the language games—associated with different linguistic "forms of life." Wittgenstein understood these matters better than most. What he and his followers seem not to understand is that the scope of those who share a given language and its meanings may be far more limited than they have imagined, that there may be a multiplicity of languages and forms of life within a given society, even among those who use similar words. Yet this multiplicity is not reflected in theorists' language. Self-conscious analysts and advocates of difference move as easily as defenders of the canon to a detached linguistic site, writing in a strong and confident, sophisticated voice that presumably comprehends and speaks for everyone. Theorists' habits of living and writing remain untouched, a narrow(ing) vision in the midst of a richly varied and diverse social world.

It seems worthwhile, then, to consider at least briefly the language and form of life learned and carried on by social and political theorists. Then we will be in a better position to consider whether that language reflects a form of life shared by all, whether it is able to speak for all. First, a few examples in which the language-of-theory speaks—unequivocally, masterfully, without hesitation, without doubt. In the following exemplars of theoretical writing and in those to be considered later, I want to underscore the tremendous similarity in the mode of expression, the virtually indistinguishable tone and style and structure exhibited in the writing. Always to be kept in mind, as well, is the form of life associated with those who are expert practitioners of this theoretical language, of those who readily and with analytical and professional pride write such passages as the following:

> The self becomes an individual in that it becomes a "social" being capable of language, interaction and cognition. The identity of the self is constituted by a narrative unity, which integrates what "I" can do, have done and will accomplish with what you expect of "me," interpret my acts and intentions to mean, wish for me in the future, etc. (Benhabib 1992, 5)

> Human beings who are not severely diseased or demented are regarded as desire- and belief-forming, intentional, purposive, and more or less rational. To the extent that individuals use these capabilities and are partly apprised of the considerations that have informed the actions of other persons, they are typically able to comprehend and to make pertinent responses to one another's actions. (Flathman 1992, 125)

> We are the beneficiaries of three centuries of democratic thought and developing constitutional practice; and we can presume not only some public understanding of, but also some allegiance to, democratic ideals and values as realized in existing political institutions. (Rawls 1987, 2)

> We agree surprisingly well, across great differences of theological and metaphysical belief, about the demands of justice and benevolence, and their importance. There are differences, including the stridently debated one about abortion. But the very rarity of these cases, which contributes to their saliency, is eloquent testimony to the general agreement. (Taylor 1989, 515)

These brief quotations are models of the language-of-theory, drawn from the writing of some of today's most prominent theorists. What form of life is associated with such linguistic mastery? I would note, first, a number of similarities in the voice that appears in these theoretical pronouncements. Prominent among these are strong author responsibility and detachment, confidence-in-understanding, omniscience with respect to the issues discussed, comprehensiveness, analytical rigor, and complexity. The writing employs an extensive and specialized philosophical vocabulary (expressed through a complex syntax) that makes it possible to build idea upon idea and weld together strong, precise connections. A long and often fruitless search is required to find expressions of uncertainty or doubt, hints of confusion, or just plain guesswork.[4] The sources of theoretical writing remain almost entirely the writing of other theorists—those in one's own camp for purposes of drawing support and wrongheaded folks in other camps to highlight mistakes associated with their flawed ways of thinking.

Built into this confident, precise, knowing p(r)ose is a sense of self as being-in-control that matches comfortably with the privileges and practices that comprise such theorists' lives. Each lecture and paper and monograph is a carefully scripted movement from precisely formulated assumption to painstaking conceptual clarification to fully derived principles and propositions. Avoidance-of-inconsistency with its overriding analytical ethos seems to be the formative monitor and judge that shape the sense of theoretical achievement, marking the identity of the theoretical self. And somehow the implicit or explicit assumption remains that this singularly privileged form of life in which planning and participation and perfectly scripted sentences make sense is shared by all, that the analytical writing drawn from it speaks for all.

All others are presumed to be "just like us," and their concerns can be met by carefully turning the antenna of philosophical analysis in their direction. Everyone, it is claimed, embraces basic ideals that are presumed to be the hallmark of philosophical and political development during the last three hundred years or so of modern Western societies—individual rights, representative institutions, equal opportunity, market exchange, agreement on "high" moral values. It is not that others have incommensurably different ideas of themselves or embrace different views of social or political life or speak and live by the meanings and practices of different languages. It is only that insufficient efforts have been made to include them within a discourse that they share with everyone in "modern Western societies" (Flathman 1987, 319; Rawls 1987). Marginalized people are thought to embrace liberal ideas of individual independence and rights and democracy-by-formal-consent. Those few who do not, Richard Rorty confidently tells us, are properly viewed as "mad" (1991, 187). And pretty much everyone is said to have adopted the personal and social virtues that characterize the "high" moral tradition in these societies, for if they did not, Charles Taylor assures us, "they would be at sea" and "wouldn't know anymore, for an important range of questions, what the significance of things was for them" (1989, 27).

Yet if language both reflects and carries within it the practices and meanings that comprise a form of life, nagging questions keep coming to mind with respect to the knowing, confident theoretical voice that would speak for all: Whose voice is this? What form of life does it carry? Isn't the language-of-theory falsely passing as a language that can speak for everyone? Is it not only the echoed voice of those who have been constructed by—who speak and write from—the practices and routines of the language-of-theory? The erudite, precise language indelibly imprinted onto the secure, comfortable form of life of academic theorists?

Rather curiously, this same knowing voice is also evident among poststructuralist theorists bent on deconstructing the works and ideas of major philosophers. Consider the following:

> The judges of normality are present everywhere. We are in the society of the teacher-judge, the doctor-judge, the educator-judge, the "social-worker"–judge; it is on them that the universal reign of the normative is based; and each individual, wherever he may find himself, subjects to it his body, his gestures, his behaviour, his aptitudes, his achievements. (Foucault 1979, 304)

> In the case of [Adam] Smith, his construction of the double self, one acting and one imagined as outside the actor observing from an impartial perspective, is the primary move through which he naturalizes the self's relationship to normativity. (Shapiro 1993, 121)

> Agonistic respect, as I construe it, is a social relation of respect for the opponent against whom you define yourself even while you resist its imperatives and strive to delimit its spaces of hegemony. Care for the strife and interdependence of contingent identities and the differences through which they define themselves, for instance, means that "we" (the we is an invitation) cannot pursue the ethic that inspires us without contesting claims to the universality and sufficiency of the moral fundamentalisms we disturb. (Connolly 1993, 155)

These analyses are ostensibly motivated by a concern to incorporate ideas drawn from forms of life that have resided in the gaps and erasures of the dominant discourse, from the fractured identities and unanchored, ambiguous meanings associated with those who are Other. And much deconstructive effort has gone into showing how the identities of people carrying on within those forms of life have been socially constructed in ways that are missed by dominant theoretical interpretations. Yet somehow, the deconstructive theorist is able to suggest her or his ungrounded—but nonetheless more "interesting" and analytically powerful—deconstructed interpretation from a distant social and linguistic site. In any case, it is difficult to see how the voices of the marginalized themselves speak when analyzed by the linguistic tools of deconstruction. The writing of Foucault, Connolly, Shapiro, and other deconstructionists is, if anything, even more esoterically theoretical than canonical theory. Jacques Derrida certainly moves outside linear, tightly reasoned theoretical writing. Yet it is not clear what alternative voice, what different form of life, Derrida speaks from or for; and Derrida, no less than Foucault or Connolly or Shapiro, retains a knowing analytical voice. Canonical texts and meanings provide the basis for analysis, even if the canon's major ideas and ideals are challenged; there is closure, even if a lack of closure is called for; and there is a tone of articulate certainty, even if ambiguity is recommended.

Is this surprising? Probably not. Occasional intellectual doubts are no match for years of virtuoso performances in the master language when these are reinforced by longstanding, unambiguous, at-the-center-of-things identities. Inheritors of such identities are more inclined to speak than to listen, to judge than to be judged, to allow or deny passage to others while comfortably carrying on within the protected space of social-linguistic dominance. In this space, energetic forays into the theoretical nuances of otherness can only be expected, once again, to assimilate different voices or to render them mute.[5]

While far more difficult to detect and assess, acquired habits of dominance and their skillfully crafted messages also hinder the efforts of theorists who, themselves, speak from difference.[6] Gayatri Spivak is among the most careful and reflective theorists to do so. She is cognizant of her own status as a socially constituted, postcolonial theorist as she interrogates the meanings that have been imposed on those residing in the margins of Indian society and culture under the dominance of Empire and, later, Nation. She recognizes that most theorists' drive to generalize—whether for the purpose of promoting dominant meanings or of challenging them in the name of the postcolonial marginalized, the subaltern—typically imposes the meanings it claims to discern or through which it would liberate (Spivak 1987, 1990). In a now extensive, impressively informed, and analytically sophisticated work, she exposes the layers of meaning that have been produced and recreated and imposed, over and over, on Indian cultural life. Yet this impressive work that shows the several faces of dominance also seems to carry within it a denial of agency and a perhaps unwanted assimilation imposed by another theoretically driven silencing. In her discussion of *sati* (widow suicide), for example, it is noteworthy that there seems to be no place for any agency or subjectivity on the part of the widows themselves (Spivak 1988). Spivak argues, "What I find useful is the sustained and developing work on the *mechanics* of the Other; we can use it to much greater *analytic* and interventionist advantage than invocations of the *authenticity* of the Other" (p. 294; emphasis on *analytic* mine).[7] It is both surprising and troubling that perhaps the central conclusion of Spivak's essay is that the subalterns cannot speak: their identities and their choices are thoroughly constructed; they have and can have no voice.[8] Any who would claim otherwise are said either to impose Euro-centered notions of autonomy or to appeal to a nonexistent native identity. Paradoxically, the silencing of difference under the guise of difference-as-sameness may have been replaced by a view of deconstructive difference that insists that the marginalized have no voice to silence, no voice to represent.[9]

Others' senses of themselves are analytically assimilated, as the language of poststructuralist theory and the form of life of its practicing theorists require, into the terms of endlessly open, decentered, fractured, unanchored identities. Any hint of an essential or foundational grounding is theoretically disallowed, whatever the views of those who are subjected to deconstructive analysis. Armed with the confidence of her social site and the analytical tools of her theoretical assumptions, Spivak traces the discursive mechanics through which others have been entirely constituted. It seems that others' senses of themselves and their agency need not be consulted in the theoretical project of deconstruction. Unlike those theorizing them, they are thought to have no agency to consult, no language of their own in which to speak.

Does postmodern or poststructuralist theory, even in the hands of those attentive to difference, leave any openings in its terminology of erasures and social constructions, subjects-as-effects, and fractured identities for the voices of others? Unless and until such theory joins hands with a language that respects them as possible subjects and speaks in their voices, I doubt it. I am inclined to follow Kalpana Ram in her suspicion that the analytical project of deconstructive theory "ends up being a tool of an assimilationist and universalizing drive—all the harder to name as such because of the continual celebratory rhetoric of difference, diversity, heterogeneity, and localisms" (1993, 11).

My doubts are not lessened by the writing of other strongly self-identified deconstructive theorists who, with Spivak, offer interpretations of the sense of self, ideas and ideals held by those who are Other. Donna Haraway (1991), Teresa de Lauretis (1987, 1990), and Judith Butler (1990, 1993) map the construction of subjectivity at various sites within the web of (post) modernity. The subjectivities they reveal are far removed from dominant readings: selves constituted as postmodern cyborgs, formed by but nonetheless subjective participants within the impulses of electronic technology and late-capitalist imperatives (Haraway 1991, chap. 8); selves who take on the celluloid images of film but who nonetheless retain an agency (somewhat) of their own in the "elsewhere" of those images (Lauretis 1987, chap. 1; idem 1990); selves who are en-gendered as male or female but who, knowing that gender has been fully constructed by socially prescribed norms, create a particular agency of their own through performances that move within/outside those norms (Butler 1990).

The theoretical reconstructions of de Lauretis, Haraway, and Butler argue that the Other has a capacity for agency. I wonder, though, whether any others could recognize themselves in the theoretically painted landscape. Again, consider some examples:

> One does not stand at an instrumental distance from the terms by which one experiences violation. Occupied by such terms and yet occupying them oneself risks a complicity, a repetition, a relapse into injury, but it is also the occasion to work the mobilizing power of injury, of an interpellation one never chose. (Butler 1993, 123)

> The only way to characterize the informatics of domination is as a massive intensification of insecurity and cultural impoverishment, with common failure of subsistence networks for the most vulnerable. (Haraway 1991, 172)

> If in the master narratives, cinematic and otherwise, the two kinds of spaces are reconciled and integrated, as man recontains woman in his (man)kind, his hom(m)osexuality, nevertheless the cultural productions and micropolitical practices of feminism have shown them to be separate and heteronomous spaces. (de Lauretis 1987, 26)

Analytically, these theoretical views claim to allow a place for the marginalized to speak. Yet it is uncertain whether and how any others have been consulted; for the most part, their agency is allowed and defined through the grace of analysis. What they have to say and whatever agency they retain have been determined by and passed through the language-of-theory. Again, the nagging questions come to mind: Whose language is this? From what form of life does it speak?

A number of scholars who write of and from difference, less concerned with wearing the crown of theory, share the doubts raised by these questions and have developed political ideas and ideals in writing that moves beyond theoretical language. In an effort to pass beyond the male-centered character of dominant theory, French feminists Monique Wittig (1969, 1980), Hélène Cixous (1976, 1991), and Luce Irigaray (1985a, 1985b, 1993) move back and forth between writing literature (essays, novels) and theory. This is also evident in the writing of Gloria Anzaldúa (1987), bell hooks (1984, 1989, 1990), Patricia Williams (1991), Paula Gunn Allen (1986), and Trinh Minh-ha (1989). And still others such as Cherríe Moraga (1993), Michelle Cliff (1985, 1987), Audre Lorde (1982, 1984), and Adrienne Rich (1979, 1986) have come to present their ideas entirely within literary forms truer to their own inherited and undeniably present, shifting and chosen identities.

It is impossible to comment here on the scope and depth of the ideas presented in this tremendously diverse writing. With respect to theorizing difference, however, a number of impressions come to mind. The most undeniable is simply the greater range and depth of what is written. Images of feminist identity are presented by Wittig and Cixous and Irigaray that make canonical conceptions of virtue and autonomy appear incomplete and empty. In works that are both literary and theoretical, these writers show us the masculine character of dominant theoretical conceptions of self (independence, power, aggressiveness), trace the deep penetration of that self throughout Western language and culture, and contrast this with the quite different senses of self and relationships (caring, eros, *jouissance*) that are shared by women as a result of their different languages and forms of life. In Luce Irigaray's words, "You have built an anaesthetic world. But when our greatest pains or greatest joys are abolished by calculation, is that not the worst destruction? A realm beyond pain, where suffering no longer exists" (1992, 55).

There are other impressions. The relationship between theoretical and literary writing remains uncertain. French feminists challenge ideas in dominant theories but seem uncertain whether theorizing is, in itself, an oppressive social and linguistic practice. They are both repulsed by and drawn to theoretical language, viewing it as the voice of oppression while also wanting to see more clearly the connections that produce that oppression and to engender transformations beyond oppression. And theory of some sort often seems needed to make those connections and to pursue those transformations. Thus theory appears in the background of the writing by hooks, Williams, and Allen; yet when it surfaces, its ideas seem awkward, forced, included as a matter of academic expectation, fitting poorly into ideas developed within the more literary narratives. Of course, the essays and poetry of Moraga and Anzaldúa, Rich and Lorde are often simply denied the status of theory. And such writers typically pay a price for their irreverence. Luce Irigaray was removed from her academic position for her writing (1993, 52). In literature, Carolyn Heilbrun (1988) followed George Eliot and George Sand and found it necessary for years to write under a pseudonym. Gloria Anzaldúa and bell hooks recognize that writing essays and poetry outside the analytical mold means that their work is often not taken seriously as theory.[10] Trinh Minh-ha (1989) and Cherríe Moraga (1993) write poignantly of how learning to write in the voice of academic analysis meant the loss of their own voice and, with that loss, a diminished ability to see and write about compelling issues.

Like a just-returned-from-afar, overbearing world traveler[11] in the midst of an evening's conversation, the confident analytical voice draws everyone's attention to itself, stifling others' comments in midsentence, forcing everyone first to silence and then to occasional whispers. Unless the cosmopolite leaves or manages to force himself into a quieter and more attentive way of listening and speaking, all other conversations must be carried on surreptitiously, a comment here and there, a passed note or brief remark when the self-absorbed, knowing intruder's back is turned.

A central difficulty remains. How are those "we" who would theorize difference able to gain access to some potentially imaginable "others" as we go about the tasks of theorizing? An important opening to this question has been suggested by a number of the feminists and scholars of color mentioned above. Literature and literary writing both ranges beyond the narrow scope of theoretical analysis and, perhaps, makes it (somewhat) possible to gain access to others, to the Other that ranges within ourselves.[12]

LITERARY LIFE AND LANGUAGE: PASSING BEYOND THE LANGUAGE-OF-THEORY

If not routine, the use of literature and literary forms of writing are not absent from social and political theory. Allegories and metaphors, as well as writing that is richly literary, are central to Plato's *Republic* and figure prominently in the writing of such disparate philosophers as Niccolo Machiavelli (1957, 1988), Jean Jacques Rousseau (1931, 1964, 1979), Friedrich Nietzsche (esp. 1966, 1967, 1968, 1974), Michael Walzer (1988), and Richard Rorty (1989). Such attention to literature and literary forms undoubtedly extends the reach of careful analysis and theoretical argument. Indeed, it is literary metaphors that are often the focus of theoretical praise or critique: Plato's imagery of the cave, Rousseau's story of Emile, countless Nietzschean metaphors presented in a virtuosity of literary writing.

Yet these and similar references to or the use of literature and literary forms in social and political theory are problematic in theorizing difference for a number of reasons. Perhaps the central difficulty is that with the exception of Nietzsche, this literary writing is viewed primarily as a hired hand in the work of analysis. The references to literature are brought in to help make a theoretical point. In the *Republic*, Plato's allegory of the cave and myth of the noble lie serve the development of his theory of justice; Rousseau's literary account of Emile's education is meant to teach us about citizenship; Walzer (1988) praises George Orwell and criticizes Albert Camus in ways that promote his conception of community; and Richard Rorty (1989) interprets George Orwell and Vladimir Nabokov to support his theoretical division of public and private. And what of Nietzsche? Sometimes, to be sure, his brilliant epigrams and metaphors are largely centered on cutting away the metaphysical braces which support canonical philosophical ideas and ideals. He encourages those of us living in postmodern times to forge a will, an aesthetic, within the confines of our genealogical inheritance. Much of Nietzsche's intellectual brilliance is precisely because he writes great literature that ranges courageously outside theoretical concerns and theoretical writing, that gives aesthetic expression to a form of life beyond academic security and Apollonian reason.

I have tremendous respect for these ventures beyond the language-of-theory. Without them, the history of social and political theory would be far more barren than it is. Yet others' voices may be as absent from canonical literature as they are from canonical theory. Much recent feminist literary criticism insists on this point (Christian 1985, 1989; Heilbrun 1988; hooks 1989; Jacobus 1979; Kolodny 1985; Miller 1988; Morrison 1992a; Showalter 1982). Even Nietzsche's impressive literary writing painted dismissive, disturbing pictures of those who are Other (women, the herd). But which "women" and who among the "herd" did Nietzsche consult? I doubt that their brushes ever touched the canvases on which their portraits appear.

Literature and literary writing drawn from the dominant discourse moves further into the pulses of personal and political life than detached, precise analysis; but, as Toni Morrison (1992a) so clearly shows in *Playing in the Dark*, major literature is typically written from the meanings offered by the dominant discourse. Here, too, others either have no voice or are interpreted through dominant social codes.

PASSING('S) THROUGH: MINORITY LIFE AND LITERATURE

Special difficulties are met when trying to engage minority voices more fully. Marginalized people have little choice but to speak and write in the master tongue. To the degree that they bring senses of themselves and beliefs and aspirations of perhaps radically different sorts to their use of that single tongue, minority writers must find ways of using it that express such differences. Knowing that they are using a master tongue, their different forms of life prod that tongue, move it off-center, fashion (somewhat) a tongue of their own.

Some of the dilemmas confronting minority writers who are obliged to use a major tongue are suggested by Gilles Deleuze and Felix Guattari (1983, 1986) in a discussion that focuses on the writing of Franz Kafka. Most fundamentally, writers from the margins are said to face the problem of deterritorialization. They speak and write in the major language even as it is known and felt as an alien tongue; they are viewed as alien while forced to communicate in an alien voice. Yet they learn to speak and write so that the language expresses who they are and would be. Always pursued by the master's linguistic scouts, they walk uncertainly through a thick forest of imposed and imposing words and meanings that at each step, threaten to absorb them into a dominant same-ness or expose them as lost, dreaded strangers should they move too boldly into its clearings.

In common with literature written from the dominant voice, minority literary writing[13] creates linguistic rhythms and strategies that circulate well beyond the narrow accounting of analytical exposition, beyond the "proud but calcified language of the academy" (Morrison 1994, 16). Yet Deleuze and Guattari (1983) suggest that great minority writers, who are neither captured by dominant meanings nor succumb to the fear of being exposed, create linguistic innovations that achieve a reterritorialization of language—forcing a major language to give life and breath to minority voices. This may be done through an intensification of a meaning in the major tongue that "spins it out along creative lines of escape," as in Kafka's relentless imagery of bureaucracy as seen by those within its grasp (p. 27). In addition to or in combination with such intensifications, refashioning the major language may occur through substituting a vernacular or mythic tongue (which is *here*, where a particular *we* live) for the referential tongue-of-society used by mainstream philosophers and writers (which presumes to be everywhere and to speak for all).

Speaking more from a particular literary voice than through the lens of theory, Michelle Cliff makes the point more strongly and directly: "To write as a complete Caribbean woman, or man for that matter, demands of us retracing the African part of ourselves, reclaiming as our own, and as our subject, a history sunk under the sea, or scattered as potash in the canefields, or gone to bush, or trapped in a class system notable for its rigidity and absolute dependence on color stratification. On a past bleached from our minds. . . . It means also, I think, mixing in the forms taught us by the oppressor, undermining his language and co-opting his style, and turning it to our purpose" (1985, 14).

The novels and essays of Toni Morrison are rich with language that offers creative literary intensifications, that imaginatively reveals the lives of black people in the United States—and thereby exposes repressive dimensions of the dominant discourse while providing access to the lived character of black life that have gone unnoticed by those who presume and write as though there were a single language. She begins *The Bluest Eye*, her first novel, with sentences that could have been drawn from a book which, a few decades ago, was the primer from which most American children first learned to read. "Here is the house. It is green and white. It has a red door. It is very pretty. Here is the family. Mother. Father, Dick, and Jane live in the green-and-white house. They are very happy. See Jane. She has a red dress. She wants to play. Who will play with Jane? See the cat. It goes meow-meow. Come and Play" (1970, 7).

The innocent for-whites-only images of childhood presented in the dominant discourse are undermined by a rich, haunting telling that reveals the horrors and despair associated with each sentence of a for-white-children writing. At each turn, the lines of the primer are relentlessly collapsed, anticipating the endless, violent lies they impose/hide. The chapter relating Pecola's rape by her father is boldly prefaced with

> SEEFATHERHEISBIGANDSTRONGFATH
> ERWILLYOUPLAYWITHJANEFATHERIS
> SMILINGSMILEFATHERSMILESMILE. (1970, 10)

And the chapter in which Pecola is manipulated into poisoning a dog by a self-proclaimed minister begins with

> SEETHEDOGBOWBOWGOESTHEDOG
> DOYOUWANTTOPLAYDOYOUWANT
> TOPLAYWITHJANESEETHEDOGRUNR. (p. 130)

In such ways, minority literature brings our attention to images of people who think and feel and live by genuinely different lights, who have a tangled rather than a uniform history. This writing explodes the presumption of a common language, of a theoretical language that can serve as the master speaker or translator for all tongues, of a high analytical prose that can interpret for and serve all, of a form of life that is common to all who speak the "same" language.

Those who would write political theory insist, of course, that analysis or, more loosely, inquiry must proceed further than the multiple, particular ways of seeing and believing that are held by different people. Politics does not stop at the water's edge of cultural or racial or gender particularity. The borders of difference cross and intersect; and our efforts to understand politics need to address such border crossings and to suggest principles and arrangements that amount to more than an arbitrary imposition of force. Also, the linguistic innovations of marginalized people are deeply connected to the dominant language, often to the point of being symbiotic. The ways in which a particular we(they) speak and understand is constructed, in large part, in relation to the meanings that we(they) have come to assign to them(us). The meanings carried by highly particular "final" vocabularies are interwoven with those embedded in the single, "common" language. This linguistic interconnectedness is emphasized by Toni Morrison: "As a disabling virus within literary discourse, Africanism has become, in the Eurocentric tradition that American education favors, both a way of talking about and a way of policing matters of class, sexual license, and repression, formations and exercises of power, and meditations on ethics and accountability. Through the simple expedient of demonizing and reifying the range of color on a palette, American Africanism makes it possible to say and not say, to inscribe and erase, to escape and engage, to act out and act on, to historicize and render timeless" (1992a, 7).

A language that identifies the good with light and truth with impartial reason is, at the same time, a language that associates evil with darkness and sees error as resulting from emotion, passion, and perhaps even simple caring. So dominant and marginalized voices are distinct yet inseparably joined—a joining often represented more by linguistic silencing and marginalization, internalization and assimilation, oppression and cooptation—than by mutual agreement.

Literary writing has far more potential than generic, rigorous analytical prose to reach into the deep and often unsavory (notice that the word which almost 'naturally' comes to mind here is *dark* rather than *unsavory*) forces that divide or join people. Bringing theory to difference would seem to require ways of writing that are deeply and fully connected to people's different forms of life, that imaginatively detect and engage the traces of writers' and readers' shared worlds. As noted by Toni Morrison in *Playing in the Dark*, "The ability of writers to imagine what is not the self, to familiarize the strange and mystify the familiar, is the test of their power. The languages they use and the social and historical context in which these languages signify are indirect and direct revelations of that power and its limitations" (1992a, 15).

REFLECTIVE IMAGINATION: MINORITY LITERARY WRITING AND REFASHIONING THE MASTER'S TOOLS

Unlike the language-of-theory, literary writing is able to use the full range of a language's vocabulary and syntax, to capture mood, to note inflection, to seek out what inspires, and to detect what is evaded or hidden, to reveal not only the many senses of what is but to detect the stirrings of what might become. The creative reterritorialization of language by minority writers matters for purposes of political theory. It matters in terms of the ways in which theory is written, the range of experience that informs theoretical writing, and the conceptions of important political ideas that are the heart of theoretical visions.

Consider briefly the problem of individualism. Toni Morrison throughout her novels and essays, Gloria Anzaldúa (1987), Edward Abbey (1956, 1975, 1988), and Andrea Dworkin (1986, 1990) share liberal theorists' deep concern for individual freedom, for people's ability to lead lives outside the blinding light of oppression. Yet an understanding of what it means to have found refuge from that light—of individuals who lead lives of their own choosing—takes on quite different meanings when viewed through the imagination of these minority writers. The "individuals" whom these writers see live in a highly particular *here*, not within a social and linguistic *everywhere*. Abbey writes of people whose sense of themselves has been built in relation to the austere challenge of the American Southwest's richly barren desert rock and sand, the frightening solace of its imposing mountain peaks with their precious and fragile, harsh and unforgiving beauty. Toni Morrison writes of those who know they are the targeted "others" within a social landscape of racist hatred and violence and a racially coded language but who, nonetheless, have created a way of living of their own. So, too, presumptions of or a search for the core, individual self make little sense to those who are aware of themselves as *mestizas* in the borderlands of conflicting cultures because

> to live in the Borderlands means you
> are neither *hispaña india negra española*
> *ni gabacha, eres mestiza, mulata,* half-breed
> caught in the crossfire between camps
> while carrying all five races on your back
> not knowing which side to turn to, run from
> (Anzaldúa 1987, 194).

There is an individuality within these particular, marginalized sites; but it is one in which who one is and would be is woven into the physical and/or social landscape in ways that are strongly sensed and poorly understood, yet seen as deeply a part of the self that would choose and act. Thus a concern for individuals' freedom would seem to begin with protecting those dimensions of people's physical and social worlds that they themselves have come to cherish. And what is cherished varies tremendously. Those who share Edward Abbey's self-vision embrace a natural landscape left in a wild state filled with life-threatening mountain blizzards and inescapable desert heat, Gila monsters and mountain lions and whistling swans. Abbey and his kind know that they are who they are because they have been shaped by this land—by days of hiking across Death Valley or the Great Salt Basin with no assurance of enough water, by nights spent in the open as cries of coyote and owl fill the wind, by eyes filled with rim after rim after rim of Colorado River–formed rock walls forged through eons of time. "Improve" or "develop" this land with highways and resorts and you destroy a central part of who they are and would be.

Andrea Dworkin and the women of New York's Lower East Side for whom she speaks have been shaped by fear and anger and rage that come from being relentlessly subjected to rape and the threat of rape; to winds that curve around corners and seek out thinly covered flesh huddled in exposed doorways; to rain that comes in sheets, saturates clothes, and leaves a chilling cold that never goes away. Some of her deepest desires are for a place of peace and warmth, serenity and beauty that is far from threatening streets and never-warm, never-secure apartments with their endless, wanton sexual and physical abuse. The mountains she would have would be like the Berkshires—green and rounded and peaceful, viewed through the sun-lit rooms of quiet and secure lodges. Take away this possibility, and you also deny her sense of a self and a social order within which life could be more than just the physically strong seeking out victims as lust and power demand, you carry away her vision of what makes sense and what must be resisted.

It is not that theoretical presumptions of or searches for an unambiguous individuality are empty. For at least a number of minority writers, there clearly is some sort of "individuality" lurking about. Yet minority writing reverses the foreground and background of privileged, "independent" individuality. Living in the invisible spaces that often help provide the perquisites of privilege, marginalized people cannot blink away connections between dominant social and linguistic meanings and the rhythms by which they think and act. Marginalized people's senses of the pervasiveness of those rhythms is not diminished by the language-of-theory's analytic pronouncements that they, too, share a robust autonomy, participate in a high moral tradition, or can exhibit their individuality through playful irony and subtle ambiguity. These minority voices suggest that if cherished parts of their particular stories are made secure, no one need waste analytical prose shoring up the individuality of their choices.

When theory is brought to difference, dominant conceptions of community in terms of the presumably elevated, fundamental moral values of a society's general history and culture also recede from view. The writing of Toni Morrison shows us a protective rather than a morally exalted or policing community. Blacks in the United States protect each other against incursions from whites—all whites—while offering each other comfort and support, respecting the need to suffer pain and humiliation in silent anger or violent rage or sheer madness, celebrating richness and vitality, praising and blaming without stopping to figure out precisely what deserves praise or blame. Strong judgments are few and forgiveness is given if unstated.[14]

EPILOGUE/CONCLUSION: IMAGINING THEORETICAL (IM)POSSIBILITIES

The regularly changing voices I try to hear, from which I would now write and speak, are not those of a person who clearly knows who he is nor even of a person who puts much stake in or has much pride in seeing himself—or in being seen—as a *he*. Dig all the way down into identity or subjectivity and there isn't any person-with-a-voice to be found. Like most people, I suspect, I am some mixed combination of what I have done, of what has happened to me and my reflective efforts to place all of that into coherent if temporary notions about the self and politics, into efforts to pass as and not to pass, to pass silently within and to pass imaginatively beyond. I am drawn to Mary Gordon's sense of self and of living in her story, *The Rest of Life*, which lets go of precise, theoretically driven ideas of self and the good and the right: "But how do any of us recognize ourselves? By being around familiar objects, by performing actions that have some similarity to the actions of the past. I think that's all" (1993, 6).

That's not all, of course. Yet I see Gordon's portrait of a self as showing the good sense not to say more, eschewing efforts to represent the strong but contingent connection between our selves and our habits with reductionist notions borrowed from this theory or that. There are hints of Wittgenstein in Gordon's image, even stronger ones of Foucault. I suppose that in our theorizing, such strong readings and the elaborate connections they make between who we are and where we happen to be have a place. But I wonder.

Why not leave disparate, reflective/literary accounts in place, clearly associated with the form of life that has called them up—doubting any effort to prioritize them or to analyze just which one is more basic or consistent or true by some general view. With this questioning of the need to say more—to analyze more—may come a reflective sense of self that more fully sees the richness and ambiguity and utter contingency of the rest of life, of all those parts of life that circulate within/beyond analytical ideas about true selves and what they should believe and how they should be measured, when they are free and what they are entitled to. If it seems important to say more, to trace out the forces that have shaped selves who are also agents, I suspect we should try to match the richness and simple grace of Gordon's writing. If we cannot, I doubt that we add anything to her or to anyone's literary/theoretical accounts.

What comes to mind after this somewhat unorthodox metatheoretical foray into the pitfalls and promises of taking differences seriously? My own strongest impression as I have worried over the issues raised here is a reaffirmation of Foucault's view that intellectuals who have a penchant for firmly gluing together general claims or prescriptions from carefully selected scraps of aristocratic presumption and intuition are to be regarded with great caution (1972, 126–33). I continue to think this penchant is woven into the language-of-theory and its form of life—sometimes as a whisper, sometimes as a command—as they shape the writing of self-conscious social and political theorists of widely different persuasions.

Does this mean that those who would (re)fashion theories may face the prospect of also learning to write as novelists and poets, essayists and playwrights? I suspect so, at least some of the time. If we are to proceed from metapolitical theory to political-theories-without-a-gaze, it is hard to see how passing beyond the voice of secure and confident, detached analysis can be avoided. It is also difficult to imagine that those who pass their days entirely within the form of life of privileged academic theorists can acquire much facility in using anything but theoretical language. Yet those who have little or no facility to reach imaginatively into and to write about the differences within particular people's lives are probably not to be trusted when they would fashion the principles and politics that affect those lives. Their analytical reach exceeds their imaginative grasp.

So, yes, a strong dose of imaginative, literary writing, fully employing the vernacular and mythic tongues at play in the heretofore forgotten or silenced or ignored margins of the rest of life runs through all of the theoretical (im)possibilities that I see. In the words of Audre Lorde: "We can train ourselves to respect our feelings and to transpose them into a language so they can be shared. And where that language does not yet exist, it is our poetry which can help fashion it. Poetry is not only dream and vision; it is the skeleton architecture of our lives" (1984, 37–38).

As reflective/imaginative literary theorists, we may be able to find or create, here and there and for a time, particular sites where careful attention shows us widespread agreement on the details of a specific vision. If so, I'm sure that familiar habits of close analysis will and probably should reassert themselves. If difference calls into question and moves beyond even as it is woven into dominant ways of seeing and believing, analysis and metaphor and allegory also probably need to be interwoven, each coming into play as the choreography needed to understand and transform ourselves more fully is endlessly put together and rehearsed and performed.

Devotion to a philosophy and politics that fine-tunes established principles and refines their written expression may be at an end.[15] Perhaps we should begin pursuing the openings that that improbable end now makes possible, rather than standing guard over fallen ideals or repeating warnings about the certain decay and violence that follow from ideas and arrangements that are "too" local or "too" separatist.[16] With a little imagination, perhaps such concerns can pass.

Notes

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1. I am conflating liberal and communitarian theorists because, from the intellectual vantage point of difference, they offer remarkably similar views. In intellectual league against the charges of feminism and, especially, poststructuralism, they are more and more alike in their insistence that marginalized others are "just like us," beneficiaries of certain progressive moral and political ideals within the modern Western tradition. Richard Flathman (1987) and John Rawls (1987, 1993) advance this view in promoting liberal ideals of freedom and justice; Michael Walzer (1988) and Charles Taylor (1992) do so in recommending the "high" moral values of community.

2. I mention this in recognition of those residing in the margins who know their location there is *not* a way-station, who sense with every breath and movement that this will always be their lot. From that recognition and the endlessly repeated routines that push it further and further into their consciousness, a sense of self and a sense of the character and possibilities of social life are forged that, it seems to me, bear little resemblance to the ways of considering one's self and situation that come to be held by any (such as myself) who only pass through.

3. I borrow the term from Adrienne Rich (1979, 35).

4. There are instances of writing that expresses a particular, uncertain voice among a few contemporary political theorists, although the language of analysis remains prominent in their writing (e.g., Elshtain 1981; Flax 1990).

5. The strong impetus of poststructuralist theory to assimilate the meanings of others into its own terms or, alternatively, to silence them altogether is suggested by Kalpana Ram (1993).

6. I want to be cautious here. I know that I'm entering new and uncertain ground, and my pace slows at the range of unknown and unexplored complexities that appear, on all fronts, in any thoughtful attempt to understand difference. My caution is heightened with the realization that, in what follows, I critique scholars notably marked by difference even as I am not. And I do not want to detract from the important, if still incomplete, work that has been done by many who speak of and analyze difference, because they, far more clearly than I, know difference first-hand.

7. The task of consulting the voices of marginalized, postcolonial women, particularly those who lived in earlier times, poses difficult and perhaps insurmountable problems. Despite Spivak's unassailable intention to understand dominant constructions of postcolonial women and to address their oppressions, I am led to believe that the particular language-of-theory she has mastered and its associated social-linguistic site betray her project. As noted by Kalpana Ram, also a postcolonial woman and theorist, the movement from the site of academic, middle-class Indian women to First World insti-

tutions may occur (too) smoothly, with Third World traces receding quickly into the background.

8. In a recent presentation, Spivak (1995) suggested that subalterns' inability to speak does not mean that they cannot talk. I simply don't know what this means, what it can mean. And I doubt that many of the postcolonial women for whom Spivak claims to speak either recognize or appreciate this subtle theoretical distinction.

9. Spivak's view that the subaltern cannot speak is perilously close to Richard Rorty's insistence that until marginalized people become articulate in the "common" language, the "job of putting their situation into language is going to have to be done for them by somebody else" (1989, 94). His sentiment is echoed by Susan Moller Okin: "*Committed* outsiders can often be better analysts and critics of social justice than those who live within the relevant culture" (1994, 19; emphasis original).

10. This point was made by hooks and Anzaldúa during lectures I attended in the spring of 1992. Anzaldúa spoke at the University of Minnesota, Twin Cities, and hooks, at Carleton College.

11. I adopt María Lugones' (1990) conception "world-traveling" as a way of moving across divides of difference deliberately, if with considerable uneasiness. I do so because I find little or no place in the notion of world-traveling for the marginalized. This may be a notion that (some) others embrace, but we only have Lugones's theoretical claim that this is the case.

12. For an elaboration of this point, see Kristeva 1991.

13. I will collapse minor (noncanonical) literature with minority (others') literature, using the single term of *minority literature* to cover both meanings.

14. This is strongly portrayed, for example, by Sula in *Sula*, Milkman in *Song of Solomon*, Denver in *Beloved*, and Joe and Violet in *Jazz* (Morrison 1973, 1977, 1987, 1992c).

15. I use *end* in the varied senses offered by Jacques Derrida—as an attitude of deep regret felt with the closing out of an age, as an aesthetic refusal to look back on a former time, as a celebration of the possibilities offered by new if uncertain beginnings (1982, esp. 134–36).

16. In calling for social and political theorists to come to know and to write about difference as imaginative, literary artists, I fully acknowledge my own halting, tentative, and (as yet) quite unsuccessful efforts to do so—hence the writing here. In part, this is because pursuit of a general view and absorption of my writing self into the language-of-theory has accompanied my many years as a self-identified political theorist. (Having learned to swear mightily by the age of seven and to speak in the vernacular of a rural Wisconsin farming community, acquiring facility in the high Standard English of analytical prose required considerable work.) Also, the attention to rich meanings that inhabit particular sites from which imaginative, literary writing draws is blunted by years of analytical and experiential distance. So imaginative theoretical writing may come, at best, as the meanings embedded within deeply particular sites are engaged with great care and attentiveness. As that process moves forward, I suspect that overall views of society, the divining of general principles, rigorously constructed arguments, and the language-of-theory may also be transformed through a greater attentiveness to the particular sites that comprise social and political life.

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RICHARD RORTY, LIBERALISM AND THE POLITICS OF REDESCRIPTION

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In recent years Richard Rorty has sought to develop an alternative to the familiar rationalist and natural rights "vocabularies" of liberalism. Unlike most critics of classical liberalism, however, Rorty eschews attempts to argue *against these vocabularies, and instead seeks to persuade his readers by* redescribing *the aspirations of a liberal society in a more "attractive" way. I assess Rorty's redescriptive practice through an analysis of his ideal liberal polity. I contend that although Rorty defends redescription as an alternative to "normal" philosophical and theoretical argument, his redescriptive efforts fail on their own terms: not only does it appear that there is no redescription in his descriptions, but he proves incapable of offering any insights into or exits from pressing problems in contemporary liberal societies. This, I submit, can be traced back to his unwillingness to investigate and redescribe power and power relations.*

In the essay "Method, Social Science, and Social Hope," Richard Rorty (1983) contends that perhaps the most important challenge facing social scientists today is that of reinvigorating the long dormant Deweyan ideal of social science. This ideal, which flourished in America for a period prior to the advent of behavioralism, accents above all "the moral importance of the social sciences—their role in widening and deepening our sense of community and of the possibilities open to that community" (pp. 203–4). Rorty urges pragmatists not to follow the path of figures such as Michel Foucault (who, Rorty claims, adopts the "pragmatist line" but, unlike Dewey, emphasizes the darker aspects of the social sciences, the ways they "have served as instruments of 'the disciplinary society'" [p 204]) but to devote their energies to the positive task of enlarging human solidarity, that is, the task of expanding the scope and depth of our liberal democratic community by showing us how others who do not share our particular cultural practices or form of life or who look strange or who act differently are ultimately also "one of us" (p. 203). This transformation of both our sensibilities and our sense of community is accomplished, Rorty says, not by locating something universal that binds us all together but by describing what unfamiliar people are like and redescribing what we ourselves are like (1989a, xvi). By interpreting people, cultures, institutions, and practices in ways that make us more sensitive to the details of human pain, suffering, and humiliation; by promoting an appreciation of the cardinal liberal values of tolerance, diversity, and freedom; and by fostering an awareness of the contingency of all communities—pragmatists contribute to both a "strengthening of liberal institutions" and "a renewed sense of community" (Rorty 1983, 166; idem 1986b, 13).

Now if the principal aim of this Deweyan/Rortyian vision of social science is the widening and deepening of a flexible, pluralistic, open and tolerant "bourgeois liberalism,"[1] then the means to this end lies in the somewhat vague interpretive practice which Rorty labels "redescription." As conceived by Rorty, redescription is an intellectual practice employed specifically for the purpose of radically transforming or replacing a calcified but well-entrenched vocabulary. Such a practice is necessary because, following from his neo-Wittgensteinian account of alternative vocabularies and language games, in cases where one's aim is to uproot a well-established and "time-honored" vocabulary, standard forms of argument prove invariably to be "inconclusive or question-begging" (Rorty 1989a, 9). This, he says, is because the proponents of the time-honored vocabulary always demand that any arguments against it be phrased in their vocabulary. And this implies that their opponents must show that certain features of the entrenched vocabulary are internally incoherent or inconsistent or that they "deconstruct themselves" (p. 8). But these demands, Rorty holds, can never be met, for the current vocabulary defines what is coherent, consistent, and meaningful speech in the first place (p. 9). Thus the attempt to uproot an entrenched vocabulary through *argument* is always at best inconclusive, simply because there are no non-invidious common criteria of evaluation or comparison. For this reason, Rorty maintains that "interesting philosophy is rarely an examination of the pros and cons of a thesis. Usually it is . . . a contest between an entrenched vocabulary which has become a nuisance and a half-formed new vocabulary which vaguely promises great things" (ibid.).

In these instances, what is required is not careful argument but an ability to show how things might look when rearranged and placed in a different light. As Rorty explains, this "method" of philosophy

> is the same as the "method" of utopian politics or revolutionary science (as opposed to parliamentary politics or normal science). The method is to redescribe lots and lots of things in new ways, until you have created a pattern of linguistic behavior which will tempt the rising generation to adopt it, thereby causing them to look for appropriate new forms of nonlinguistic behavior, for example, the adoption of new scientific equipment of new social institutions. This sort of philosophy does not work piece by piece, analyzing concept after concept, or

> testing thesis after thesis. Rather, it works holistically and pragmatically. It says things like "try thinking of it this way" . . . It does not try to pretend to have a better candidate for doing the same things which we did when we spoke in the old way. Rather, it suggests that we might want to stop doing those things and do something else. But it does not argue for this suggestion on the basis of antecedent criteria common to the old and new language games. For just insofar as the new language really is new, there will be no such criteria. (1989a, 9)

In short, redescription is not an attempt to enunciate "the right description" but an attempt to avoid or dissolve intractable problems, conflicts, or anomalies by reweaving the fabric of our current ways of speaking into a new vocabulary, one "that cuts across the vocabulary we have so far used in our . . . deliberations" (Rorty 1989a, 99; idem 1990b, 638–39). The aim of the redescriber is not to offer arguments against the currently familiar vocabulary, but to "make the vocabulary I favor look attractive by showing how it may be used to describe a variety of topics" (Rorty 1989a, 9). If successful, the redescriber will contribute to intellectual progress by creating new metaphors and modes of speech that over time succeed in becoming literalized (p. 44).

Because the practice of redescription is resolutely not an attempt to engage in, but to avoid, "normal" philosophical and conceptual argumentation, efforts to evaluate it on these grounds are in Rorty's view fundamentally misconceived. They are misconceived for the obvious reason that they presuppose precisely the normal vocabulary and practices of justification that the redescriber seeks to circumvent and replace. As one commentator has noted, to ask Rorty "for a conceptually adequate account of redescription would be to demand a philosophically responsible account of irresponsibility" (McCumber 1990, 8). While I would maintain that there are indeed cases in which it *is* possible to argue for or against the introduction of a new vocabulary,[2] here I am primarily interested in examining Rorty's redescriptive practice in its own pragmatic terms—that is, I want to see to what extent his redescriptive efforts promise to preserve and extend freedom and pluralism, help us to determine which social and political traditions and practices should endure and be extended and which may need reconstruction or abandonment, and provide us with an attractive and promising exit from particular conflicts and dilemmas. In what follows, therefore, I will explore what is perhaps Rorty's most ambitious effort at redescription, his attempt to redescribe liberalism by sketching a picture of what he calls a "liberal utopia." By examining some of the problems implicit in this particular exemplar of redescription, I hope to identify some of the limits of Rorty's redescriptive practice. However, before examining these questions, it is first necessary to describe in more detail Rorty's understanding of current impasses in social and political thought, as well as his vision of a postmetaphysical liberal utopia.

REDESCRIPTION APPLIED: RORTY'S LIBERAL UTOPIA

Rorty's postmetaphysical redescription of liberalism is, among other things, an attempt to offer a way out of what he sees as a prototypical case of philosophical deadlock in contemporary social and political theory, namely the stalemate regarding the relation between the private and public domains of life, or, as Rorty redescribes it, the conflict between the desire for private autonomy and self-creation and the desire for solidarity and social justice. Rorty contends that these conflicting impulses constitute the principal tension between two distinct and ultimately irreconcilable types of thinkers, each with its own philosophical vision, as well as its own understanding of language, selfhood, community, and freedom: *ironists*, such as Nietzsche, Proust, Heidegger, and Nabokov and *liberals*, such as Marx, Mill, Dewey, Habermas, and Rawls (1989a, xiv).

Rorty begins by noting that in at least one respect both kinds of writers are quite similar: both are "historicist" in the sense that they reject any and all attempts at theoretically, theologically, or metaphysically grounding our "most central beliefs and desires" (1989a, xv). Like Rorty, both types repudiate the idea "that there is any such thing as 'human nature' or the 'deepest level of the self'," as well as the notion that there is "an order beyond time and change which determines the point of human existence and establishes a hierarchy of responsibilities" (pp. xiii, xv). To the contrary, they both "insist that socialization, and thus historical circumstance, go all the way down—that there is nothing 'beneath' socialization or prior to history which is definitory of the human" (p. xiii). In this respect, both have helped release us from the grip of "theology and metaphysics" and have prepared the way for a future, "postmetaphysical culture," one organized around "an endless, proliferating realization of Freedom, rather than a convergence toward an already existing Truth" (pp. xiii, xvi).

However, apart from their common animus to the idea of metaphysical grounding, these two types of historicist writers are motivated by dramatically different desires. For ironists like Nietzsche, Heidegger, and Foucault, "the desire for self-creation, for private autonomy, dominates" (1989a, xiii). These writers are interested primarily in the quest for "private perfection," in the self-creation of a distinctive, autonomous life, one that is not, as Harold Bloom says, either "a copy or a replica" (quoted on p. 24). As such, they are exemplars of "what private perfection . . . can be like"; but at the same time they distrust processes of socialization, which they tend to view as "antithetical to something deep within us" (p. xiv)—such as the will to power, libidinal impulses, or Being. By contrast, liberal authors like Mill, Dewey, Habermas, Rawls, and Isaiah Berlin are inspired primarily by "the desire for a more just and free human community" (ibid.). They see their work not as a personal quest for autonomy, but as "a shared, social effort—the effort to make our institutions and practices more just and less cruel" (ibid.). Indeed, Rorty (borrowing Judith Shklar's definition of "liberal") calls these figures liberals precisely because they "think that cruelty is the worst thing we do" (p. xv, see Shklar 1984, 8; idem 1989). However, this overriding regard for social justice and the cessation of cruelty places them deeply at odds with their ironist counterparts, whose pursuit of private perfection seems frequently imbued with a decidedly antiliberal proclivity for "'irrationalism' and 'aestheticism'" (Rorty 1989a, xv). For this reason, liberals like Rawls (in his early writings) and Habermas continue to search for a postmetaphysical anchor for their politics, hoping in this way to harness the threat of unbridled ironism.

In discussing this oftentimes acrimonious conflict between "writers on autonomy" and "writers on justice," Rorty holds that the standard philosophical solutions all seek to reconcile these antagonistic stances by uniting them under a single, more synthetic theoretical or philosophical view, one that "would let us hold self-creation and justice, private perfection and human solidarity, in a single vision" (1989a, xv). Hence, we find debates among neo-Kantian rationalists like Habermas, neo-Nietzschean anarchists like Foucault, communitarians like Michael Sandel, and philosophically oriented liberals like Rawls and Ronald Dworkin—all seeking to commensurate opposing claims by bringing them under a more synoptic philosophical view. Rorty maintains, however, that these manifold proposals are plausible only if one first assumes that the ironist and liberal visions are not fundamentally "incommensurable" but are merely *opposed*. But, he insists, they *are* incommensurable: "The vocabulary of self-creation is necessarily private, unshared, unsuited to argument. The vocabulary of justice is necessarily public and shared, a medium for argumentative exchange" (ibid.). Because of this basic and ineradicable incommensurability, there "is no way to bring self-creation together with justice at the level of theory" (ibid.).

Rather than endeavoring to unify these forever incommensurable stances in a single philosophical vision or to terminate the conflict by granting priority to the demands of one side over those of the other, Rorty instead recommends that we look at the relation between these two types of writers "as being like the relation between two kinds of tools—as little in need of synthesis as paintbrushes and crowbars" (1989a, xv). One kind of writer, the ironist, teaches us that there are indeed legitimate virtues other than social virtues, that some people do succeed "in re-creating themselves" (ibid.). These writers serve us by bringing to light our own "half-articulate need" for self-transformation and by encouraging us to become "one whom we as yet lack the words to describe" (ibid.). Conversely, liberal writers prompt us to recognize the gap between the commitments embodied in "the public, shared vocabulary we use in daily life" and the actual character of our current institutions and practices (ibid.). By pointing out the ways in which these institutions and practices fall short of our implicit commitments, these writers help give focus to our "sense of obligation to other human beings" (p. 68).

As a way of preserving what is most valuable in both types of writers, Rorty advocates not a theoretical solution but, rather, a satisfactory practical "compromise." In this compromise, each party is recognized as being "right" but only within a particular domain (Rorty 1989a; xiv–xv, 68). The challenge, Rorty believes, is to reconcile ourselves to the fact that our "final vocabulary" (i.e., those fully contingent and foundationless but nevertheless irreducible and authoritative words that constitute the linguistic ground for all of our claims about knowledge, morality, and the good life [1989a, 73; idem 1992, 216]) contains "two independent parts," one crucial for the private project of self-creation, the other indispensable for the public project of human solidarity (1989a, 68). Our imperative, he emphasizes, is not to unify but to *accommodate practically* these two "equally valid, yet forever incommensurable" parts (p. xv; see also p. 68).

For Rorty, the most suitable modus vivendi is one that grants the ironist's demand for autonomy and self-creation yet insists that the pursuit of this goal be strictly a private affair. Ironists, Rorty writes, are figures who are defined in part by their acute awareness of the arbitrariness and contingency of their own final vocabulary. Unlike nihilists, ironists have commitments, but unlike metaphysicians, their commitments are wedded to "a sense of the contingency of their own commitment" (Rorty 1989a, 61). Indeed, precisely because ironists are "never quite able to take themselves seriously," they continually entertain and experiment with other vocabularies, hoping through this ongoing process to fashion an increasingly self-made self, one that is not simply a reflection or effect of one's predecessors, historical circumstances, or local culture (p. 73). However, while this unending process of questioning, redescribing, and reweaving one's inherited vocabularies and exploring and creating new ones is central to the ironist's quest for an ever more autonomous, self-created final vocabulary, Rorty denies emphatically that this activity plays any positive role in public life. "Irony," he states, "seems inherently a private matter," something that "is of little public use" (pp. 83, 120). More importantly, Rorty warns that when nonliberal ironists such as Nietzsche, Heidegger, and Foucault seek to bring their redescriptive practices into the public domain, they typically abandon their ironic appreciation of the contingency of their own vocabulary and instead become convinced that they have hit upon some deep truth about the way in which public institutions repress inherently the desire for autonomy. Failing to recognize that autonomy "is not the sort of thing that *could* ever be embodied in social institutions," ironist *public* philosophers are led to dismiss too easily both liberalism and its attendant institutions (p. 65). When this happens, public ironists become "at best useless and at worst dangerous" (p. 68).

In order to circumvent these potential hazards of public ironism, Rorty offers an elegantly clear and simple prescription: we should "*privatize* the Nietzschean-Sartrean-Foucauldian attempt at authenticity and purity" and enforce this by insisting upon "a firm distinction between the public and the private" (1989a, 65, 83). On this view, the ironist's desire for autonomy and self-creation, as well as the utopian vision of a culture characterized by an endless proliferation of alternative descriptions (what Rorty calls a "poeticized culture"), are all to be welcomed as legitimate and even exemplary private ideals. As long as they "do it on their own time—causing no harm to others and using no resources needed by those less advantaged," ironists are free to be "as privatistic, 'irrational' and aestheticist as they please" (p. xiv). Such freedom from coercion is not only tolerable but is, Rorty claims, "the aim of a just and free society" (p. xiv). In the public realm, however, things are altogether different. This is the realm of what Wilfred Sellars calls "we-intentions," of shared practices and self-interpretations that define us not as individuals but as part of a larger moral community (quoted in Rorty 1989a, 59–60). Here our paramount concern is with public issues of social justice, and in public matters of this sort, discourse must begin necessarily from within a common vocabulary, one that permits both argument and rational consensus. This, of course, does not imply that public vocabularies must remain forever immune from criticism, expansion, and transformation but it does entail a "mild ethnocentrism," one based on the idea that public discourse must start from "the way *we* live now" and that "people [and communities] can rationally change their beliefs and desires only by holding most of those beliefs and desires constant" (Rorty 1989a, 197, idem 1991a, 29, 212).

For "us" citizens of "the secular modern West," this shared and public vocabulary is unavoidably that of liberal democracy, with its characteristic accent on averting cruelty and "the humiliation of human beings" (Rorty 1989a, xv; idem 1991a, 29).[3] Rorty openly accepts the idea that these core commitments can never be buttressed by appeals to transcendental or ontological arguments but maintains that such philosophical reinforcements are not needed in the first place. All that is required to sustain a commitment to liberal democracy is a comparative historical narrative about the way in which its customs and institutions have, on the whole, made these societies less cruel and more free (i.e., tolerant of a greater range of self-expression and providing more leeway for people to pursue what private projects they wish) than other, nonliberal societies (Rorty 1989a, 68; idem 1991a, 29). Such an account could not, he acknowledges, rationally convince just anyone of liberalism's superiority, for unless one already shares our moral sensitivity to acts of cruelty and humiliation, as well as our appreciation of the pleasures devolving from freedom, diversity, and toleration (i.e., unless one already accepts some of the "words" in liberalism's "final vocabulary"), such comparative accounts have no persuasive force. This absence of ultimate ground, however, is in Rorty's view no cause for despair not only because the demand for a "noncircular" justification of our social practices is itself incoherent but, more notably, because our own ungrounded ground is all we need for *our* social deliberations.[4]

While at first reading this resolutely nominalist, historicist, and "postmodernist" conception of liberalism might appear to be hopelessly at odds with the standard canon of liberal thought, Rorty contends that in fact it remains in tune with the basic commitments both of J. S. Mill's liberalism and Jeffersonian democracy (1991a, 175–96). For example, he tells us that although the *practical tasks* of reducing cruelty and of balancing public and private commitments require perpetual deliberation, discussion, and social experimentation, his "hunch" is that "Western social and political thought may have had the last *conceptual* revolution that it needs. J. S. Mill's suggestion that governments devote themselves to optimizing the balance between leaving people's private lives alone and preventing suffering seems to me pretty much the last word" (1989a, 63). On the question of how best to close the divide between liberal ideals and the oftentimes depressing (and cruel) realities of life in contemporary liberal states, Rorty rejects the idea that what we need is a "radical critique" of liberal thought and instead urges that "contemporary liberal society already contains the institutions for its own improvement" (1989a, 63; idem 1990b, 633–43; idem 1991b, 129–39). Indeed, the "only way to avoid perpetuating cruelty within social institutions" is simply to continue extending those institutions emblematic of liberalism, that is, to continue "maximizing the quality of education, freedom of the press, educational opportunity, opportunities to exert political influence, and the like" (idem 1989a, 66–67).

Seen in this light, Rorty's fully developed ideal polity (i.e., his "liberal utopia") is a clear repudiation of both Habermas' (1987) attempt to eradicate all ironism and ground social democratic practices in a universalist, "communicative reason" and Foucault's putative hope for a "total revolution" that would embody our autonomy in our institutions (Rorty 1989a, 65). Rather, what emerges is a dualistic, compartmentalized vision not only of the self but also of the relation between the public and private spheres. On the private side of this divide is the ironist's preoccupation with autonomy and passion for continual redescription—both esteemed as exemplary impulses in the quest to achieve private perfection. Here we find the realm of negative freedom and the absence of other-regarding obligations and hence also the realm of fantasy, play, incommensurate metaphors, and self-creation. On the public side of the divide is the liberal's commitment to the minimization of pain, cruelty, and humiliation through the expansion and refinement of "the institutions of bourgeois liberal society" (ibid., 84). This is the realm not of idiosyncracy and aesthetic invention but of "solidarity" and "we-intentions." Here the highest virtues are those of the public citizen, the person engaged in common public discourse about ways to minimize cruelty, achieve social justice, and extend human solidarity. As a polity populated by citizens who have dispensed with attempts to unify or synthesize philosophically the public and private spheres, Rorty's utopia stands as a picture of a transformed culture freed of such Enlightenment neuroses, one that seeks only to devise various practical and political measures to balance these "equally valid" yet dualistic commitments (p. xv). Likewise, the ideal resident of this polity, the "liberal ironist" (an unlikely combination of the private ironism of Proust and Derrida and the public liberalism of figures such as Dewey and George Orwell) is one who abjures the Enlightenment-induced longing for a unified and centered self and instead strives merely to negotiate the irreconcilable demands between "a private ethic of self-creation and a public ethic of mutual accommodation" (Rorty 1986a, 12).

For Rorty, this redescription of Enlightenment liberalism terminates in a new and more compelling vision both of liberal politics and of the relation between the public and private spheres. By abandoning the now-outdated vocabularies of theology, metaphysics, and foundationalist philosophy, Rorty believes that we provide the conditions for a revitalized public and private life, one that consists of an "intricately textured collage of private narcissism and public pragmatism" (1991a, 210). Moreover, he insists that the absence of such vocabularies does not leave us without the tools necessary for defending liberal commitments to freedom, tolerance, pluralism, and the like (as some critics maintain it does) but rather reinvigorates those commitments precisely by underscoring in detail both their practical advantages *and* their historical contingency. Such a story may lack the sense of epic drama found in the "grand" treatises of Enlightenment philosophy, but it is no less capable of engendering a commitment to human solidarity.

This vision of a liberal society founded entirely upon "our loyalty to other human beings clinging together against the dark, not our hope of getting things right" (Rorty 1983, 166) is unquestionably one of the more ambitious efforts to rejuvenate liberalism's most attractive ideals—namely, its moral commitment to toleration, pluralism, and the avoidance of cruelty and its preoccupation with developing a mix of rights, liberties, and institutions that protects individuals and minorities against the all-too-familiar dangers of absolutism, totalitarianism, and other forms of unrestrained statism. By detaching these aspirations from the abstract and rigid philosophical structures in which they have frequently been encased, Rorty proposes a vision of liberalism that departs both from classical social contract and natural rights versions and from those offered by contemporary foundationalist liberals like Ronald Dworkin (1977), Robert Nozick (1974), and the early John Rawls (1971). However, apart from the evident originality of Rorty's liberal utopia, one must still pose the basic pragmatist questions: What is the cash value of this redescription? Does it offer us a *useful* alternative to foundationalist liberalism, one based on a clear understanding of "the patterns of the past and the needs of the present" (Rorty 1987, 11)? Or, by contrast, does the very way in which he seeks to escape from the pathologies of conventional foundationalist philosophy leave him without the resources required for expanding and ultimately realizing liberalism's most admired ideals?

CONTINGENCY, SELF-CREATION AND CHANGE

Perhaps these issues can be defined more sharply if we begin by examining what is clearly *the* connecting thread running throughout Rorty's narrative, the rather elusive but frequently invoked notion of *contingency*.[5] In its broadest sense, Rorty construes *contingency* as the idea that things and events "might have been otherwise" (Dreyfus and Hall 1992, 18). Understood in this way, the term *contingency* typically stands at one end of a set of oppositions whose other term is variously *necessary, essential, intrinsic,* or *unconditional.* These latter terms, (as I have indicated) are themselves tightly intertwined with notions of a common "human nature," a *telos,* a divine order or some other principle of legitimacy that is privileged precisely because its status is independent of our particular historical location and social practices. At a minimum, then, Rorty's accent on contingency is tied closely to his denial of the idea that there is anything "'beneath' socialization or prior to history which is definatory of the human" (1989a, xiii).

Unfortunately, this in itself is not highly informative, for it is at best an extremely general and purely negative construal. When Rorty speaks of contingency in a more specific manner, however, he typically uses the term in one of two ways. First, he uses it in a way that links it to notions of novelty, innovation, originality, and creativity. In these instances (common in his chapter "The Contingency of Selfhood"), *contingency* is meant to pinpoint some domain or space that is devoid of any immanent nature or logic and therefore also open to innovation, transformation, and redescription. Here *contingency* represents the abundant possibilities inherent in the recognition of historicity: because our inherited practices and forms of life are not ontologically fixed but are culturally and historically constituted, they can (although never all at once) be questioned, transformed, and redescribed. Understood in this way, Rorty's invocation of contingency moves between a restatement of the undeniable fact that human beings and human history are something more than the combined effects of culture and nature and a voluntarist notion that the only impediments to human change and transformation are those set by the human will itself.

On the other hand, Rorty frequently uses the term *contingency* in a quite different manner, identifying it not with the powers and possibilities of human innovation but with notions of chance, luck, accident, randomness, and fortuitousness. This connection is made explicit in a number of different passages: "Poetic, artistic, philosophical, scientific, or political progress results from the accidental coincidence of a private obsession with a public need" (1989a, 37); Galileo "just lucked out" (1983, 193); "The idea of human solidarity is simply the fortunate happenstance creation of modern times" (1989a, 68); the mention of "random factors" and constellations (p. 17; 1986a, 12); and the use of Philip Larkin's metaphor of the "blind impress" (1989a, 141).[6] The connection is also implicit in Rorty's radically decentered conception of human subjectivity, as well as in the recurrence of haunting and despondent passages on the theme of human powerlessness. Whether explicit or implicit, however, the meaning of contingency in all of these passages is essentially the same: it implies uncontrollable and unpredictable forces or events that shape our lives in decisive ways.

What is significant about these construals of contingency is the way in which they accent Rorty's own equivocations regarding the potentialities and limits of individual and collective action. As we can see from his dual use of the term, Rorty vacillates between an "anything goes" vision of human agency in which our capacities for personal or social transformation are limited only by the powers of our individual or collective imaginations and a vision in which efforts to shape one's self and one's world are every bit as uncontrollable as in the most deterministic and totalizing philosophical systems. Here Rorty finds himself locked in the same dichotomy between voluntarism and determinism that he associates with metaphysical and foundationalist enterprises.[7]

In fact, neither of these construals are typical of the situations that characterize the better part of everyday life. Our capacity for self-creation or imagination is *not* unlimited; it is instead partly constituted, and therefore also partly constrained, by past and present social, cultural, and linguistic practices. These practices not only privilege certain imaginative and creative efforts over others but also partly constitute the categories of "novelty" and "originality," thus distinguishing them both from "the old" and from eccentricity, insanity, silliness, the quixotic, and so on. Indeed, as many commentators have pointed out, the absence of any "great" women artists or philosophers in the standard canon of Western culture reveals more about the ways in which categories like originality and genius have been constructed than about the artistic or intellectual talents of women.[8] Conversely, it may be true that there are countless unanticipated and uncontrollable events that in one way or another shape the course of our lives, but precisely *what* effect they have on us and *how* we respond to them is neither fully predetermined nor entirely a matter of "chance." Rather, these things are delimited both by material forces and by the horizons of our individual and social self-understandings—horizons that, importantly, simultaneously make meaning possible and limit the possible domain of meaning.

Rorty, of course, is not entirely unaware of these problems. He acknowledges at times that our creative capacities are not unlimited and that even purely chance events can be understood and confronted in different ways. He tells us, for example, that there are "no fully Nietzschean lives, . . . no lives which are not largely parasitical on an un-redescribed past and dependent on the charity of as yet unborn generations" (1989a, 42). Unfortunately, Rorty fails fully to appreciate the social and political implications of this insight. For if novelty and imagination are themselves partly constituted by language and social practice and if (as I have indicated) language and social practice place flexible and contestable but nevertheless real boundaries upon what is and is not intelligible, who has the right to speak, whose speech and what forms of speech are taken seriously, what counts as a problem and what counts as a legitimate solution to some problem, then questions about the pursuit of and capacity for self-creation and change are not just personal and private questions, nor are they just questions about whether we should "drop" worn-out vocabularies and metaphors in favor of newer, more useful ones. Indeed, precisely because what counts as a "worn-out" or "useful" vocabulary is itself partly constituted by the social and linguistic practices of our communities, questions about self-creation, change, and the usefulness of vocabularies all presuppose a specific social, political, and ideological context that cannot be erased even if it goes unnoticed.

This discussion of contingency and the public/private dichotomy suggests a profound and unresolved tension in Rorty's redescriptive efforts. Although his accent on contingency, along with his rigid separation of the public and the private, are both intended to open up spaces for increased pluralism, novelty, play, self-creation, and human solidarity, they tend instead to pass over or mask just those forces which not only limit the range of possible projects but also structure the level, quality, and possibility of participation in cultural and political conversations. If we ask what features within Rorty's narrative encourage these problematic construals, we find at least two interrelated sources of significance, one deriving from Rorty's understanding of politics and the political and one (which is more "methodological") concerning the relation, or absence of relation, between theories and narratives and the practices that they seek to inform.

Starting with the second, one of the most puzzling features of Rorty's redescriptive practice is the conspicuous gap between his *formal* pronouncements regarding the heuristic value of particular genres, and the *content* of his own writings. Formally, Rorty tells us that certain sorts of books are particularly "relevant to our relations with others, to helping us to notice the effects of our actions on other people" (1989a, 141). These are, first, books that "help us to see the effects of social practices and institutions on others" and, second, "those which help us see the effects of our private idiosyncracies on others" (ibid.). Beyond these very general categories Rorty places few restrictions (apart from the stipulation that theories and treatises are *not* well suited for these tasks) upon the types of books that might illuminate these issues. Apparently anything from "the reports . . . of government commissions" to novels like *Sister Carrie* and *Black Boy* are possible candidates for moral and political edification (ibid.). In accord with his antitheoretical, antiuniversalist posture, however, he emphasizes the import of narratives that focus upon particular exemplars or offer what Clifford Geertz calls "thick description." He observes, for example, that "ethnography," "concrete examples," "detailed historical narratives" and "detailed description of what unfamiliar people are like and . . . redescription of what we ourselves are like" are all particularly effective ways of enlarging our moral sympathies and political understanding (p. xvi; idem 1991b, 175).

What is significant here is not just the observation itself (which is, after all, common among those who question the practical benefits of transcendental theorizing) but rather the extraordinary disjunction between these recommendations and Rorty's own narrative practice. For within the corpus of Rorty's writings there is almost *no* "detailed description" of "the effects of our social practices and institutions on others" and there are no "detailed historical narratives" mapping the genesis and effects of those practices. In fact, it is striking that while Rorty acknowledges the existence of deep and disturbing social problems (e.g., "the unending hopelessness and misery of the lives of the young blacks in American cities" [1989a, 191]), he never describes in any detail the broader context or "social field" in which those problems are embedded, nor does he locate particular social practices that contribute to and sustain these problems.

This failure to offer any detailed description of the social field or particular social practices leads inevitably to a number of difficulties. It sustains, for example, Rorty's problematic construal of contingency. As we have seen, when he asserts that "man is always free to choose new descriptions (for, among other things, himself)" (Rorty 1979, 362, n. 7) or when he speaks of poets and other original thinkers inventing radically novel and incommensurate metaphors that eventually become literalized, he verges on the adoption of an extreme form of voluntarism—one in which, as Roy Bhaskar puts it, we are "always free to choose *any* description" (1989, 176–77). This stance appears credible *if* we examine only the most palpable and formal types of social constraints. But once we observe in narrative detail the role that linguistic and social practices play in *constraining, disposing,* and *directing* (even if not *determining* and *compelling*) our descriptions and redescriptions, Rorty's voluntaristic understanding of contingency and choice appears both facile and complacent.

To take one example, the sociologist Pierre Bourdieu, in a study of modes of classification in French academic institutions, examined the individual files of a professor of philosophy at a *prèmiere supérieure* in Paris (1988, 194–225). Through the inspection of grades and comments on the written and oral work of students of different social origins, Bourdieu found "a simple and clearly visible relation" between a *hierarchy of epithets* (evaluative comments ranging from "simplistic," "silly," and "insipid" to "lively," "cultivated," and "masterly") and a *hierarchy of social origins,* a ranking based on "the importance of the cultural capital" that students inherited from their parents (determined by, e.g., the residence and profession of the pupils' parents) (p. 197). Students from the middle classes (there were no students from the lower classes, nor any male students, since the files were taken from an all girls' school) and the provinces "were the prime target of negative judgments—and of the most negative among them, such as simplistic, servile or vulgar. . . . Even the virtues which are attributed to them are negative too: academic, painstaking, careful, conscientious" (pp. 198–99). By contrast, students from the class with the most cultural capital "almost entirely avoid the most negative judgments, even in their euphemistic forms, as they do the petty-bourgeois virtues, and they most often find themselves granted the most sophisticated qualities" (p. 199). Moreover, he found that in those instances where students from different social origins received equivalent grades, the remarks were "all the more severe and more brutally expressed, less euphemistic, as the social origins of the pupils decrease" (ibid.).

As Bourdieu notes, not only technical aptitudes, such as the capacity to construct an argument or to grasp the specialized vocabulary of particular authors, but also personal and physical qualities constitute part of the disparate criteria of professorial judgment. Especially in students' oral work, these latter "'external' criteria" become prominent, and here too there is a tight connection between students' social origins (as expressed in accent, body language, and style of speaking) and the professor's remarks on their work and talents:

> The "external" criteria, most often implicit and even rejected by the institution, have even greater importance in the remarks on oral work, since the criteria already mentioned are compounded with all those concerning speech, and, more specifically, *accent, elocution and diction,* which are the surest, because the most indelible, marks of social and geographical origins, *the style of the spoken language,* which can differ radically from written style, and finally and above all the *bodily 'hexis',* manners and behavior, which are often designated, very directly, in the remarks. (Bourdieu 1988, 200)

Bourdieu's point is clearly not that philosophy professors or professors from other disciplines self-consciously conspire to reproduce in the academic field the social hierarchies characteristic of the society in which they live. Indeed, if this were the case, the process could hardly be sustained in the way that it is (Bourdieu 1988, 207–8). Rather, he claims that it is the very way in which the academic field itself is structured that "makes it unthinkable" for both professors and their pupils to recognize "the social significance of the judgments" (p. 205). Hence, both the manifestly brutal epithets used to describe pupils' work (epithets that, Bourdieu rightly observes, "would not be permissible in ordinary usage") and the "complacency and freedom of symbolic aggression" are possible only because they operate in such a way as to appear to be something other than they are (ibid.). As Bourdieu remarks,

> It is because they think that they are operating a purely academic or even a specifically "philosophical" classification . . . that the system is able to perform a genuine *distortion of the meaning* of their practices, persuading them to do what they would not do deliberately for "all the money in the world." It is also because they believe that they are making a strictly academic judgment that the social judgment which is masked by the euphemistic implications of academic (or more specifically, philosophical) language can produce its characteristic effect. . . . The transmutation of social truth into academic truth (from "you are a petty bourgeois" to "you work hard but lack brilliance" [*vous étes travailleur mais pas brillant*]) is not a simple game of writing which has no consequence but an operation of social alchemy which confers on words their symbolic efficiency, their power to have a lasting effect on practice. (pp. 207–8)

This example shows that although Rorty would like to separate clearly private projects of self-creation from public projects of social justice, in the realm of everyday life these projects inevitably spill over and causally intermix with one another. For the middle-class student from the provinces whose project of self-creation includes the desire to teach philosophy, these concerns are neither abstract nor trivial nor easily eradicable. Indeed, they are the sort of thing of which deep personal conflicts are made, for in this case it may involve either disavowing or eradicating aspects of her identity or history or—having come to believe that she genuinely is "lacking philosophical talent"—deciding that her preferred project is not one for which she is suited (Bourdieu 1988, 206; see also p. 208). In either instance, however, the private project of self-creation cannot be neatly distilled from the social practices, mechanisms, hierarchies, and power relationships that operate, sometimes inconspicuously and sometimes quite nakedly, in the public domain. To the contrary, private projects are always and unavoidably structured by public forces.

Rorty would no doubt agree that in practice the line between the private pursuit of self-creation and the public pursuit of social justice is often fuzzy and that one of the principal aims of social science is to clarify this fuzziness by identifying previously unrecognized forms of cruelty in public institutions that constrain the private pursuit of self-creation. Nevertheless, he would—and does—insist that it is important to think of these two spheres as distinct and "forever incommensurable" universes and to make this the basis of our political deliberations. This response, however, fails to address the crucial question of why the public and private spheres *should* be viewed as linguistically and conceptually exclusive and incommensurable categories if, as Rorty himself recognizes, history and human experience clearly indicates that they are not? In other words, if, as writers such as Carole Pateman argue, the evidence of history and everyday life shows that these two spheres have *never* been simply divided but have always been intimately and "inextricably interrelated" as well (Pateman 1989, 121–22), then the proper starting point for deliberating about these spheres would seem to be precisely this evidence, rather than the artificial and abstract separation that Rorty defends. Indeed, it appears that in Rorty's effort to correct those writers who seek to unify the public and private spheres via an abstract theoretical synthesis, he simply inverts the problem by positing an equally misleading and abstract separation—one that, far from being a *redescription* of the public and private spheres, merely follows the time-honored liberal practice of insisting solely on a division of these spheres, while ignoring their historical and conceptual interrelations. The result is a description that is not in any substantive way a redescription and a proposed solution/compromise that remains detached from the political realities that it seeks to inform.

This, of course, does *not* imply that the public/private distinction has no meaningful social or political purpose but rather that philosophical solutions or pragmatic redescriptions that clearly contravene the lessons of history and everyday experience should be received with skepticism. This is especially the case when those solutions or redescriptions are ones that reenshrine patterns of thinking that have historically preserved and legitimated patriarchical or other oppressively hierarchical forms of social relations. As many feminists have observed, however, the classical liberal dichotomy between the public and the private spheres (founded on the idea that these two spheres are ontologically separate and unrelated, "but equally important and valuable" [Pateman 1989, 120]) has served historically as a mainstay of patriarchical relations in both the public and private spheres.[9] Moreover, to the extent that this philosophical separation eventually became internalized in individual and social bodies, it had, as Pateman points out, the mystifying and ideological effect of delegitimizing those aspects of experience which denied the separation (p. 131). For these reasons, many feminists have advocated a conception or ideal of the social order that is markedly different from the classical liberal and Rortyian versions. This conception

> looks toward a differentiated social order within which the various dimensions are distinct but not separate or opposed, and which rests on a social conception of individuality, which includes both women and men as biologically differentiated but not unequal creatures. Nevertheless, women and men, and the private and the public, are not necessarily in harmony. Given the social implications of women's reproductive capacities, it is surely utopian to suppose that tension between the personal and the political, between love and justice, between individuality and community will disappear with patriarchical-liberalism. (p. 136)

The preceding analysis demonstrates the way in which Rorty's reluctance to examine the complicated interweavings of past and present social practices leaves him without any analytic resources for either assessing the costs of those practices or indicating how we might preserve, extend, criticize, or challenge them. All of these endeavors presuppose that we first have some understanding of how social and linguistic practices interact; of the various privileges, distinctions, and distortions that they enshrine; and of the role they play in constituting our individual and collective self-interpretations. This, of course, does not necessarily imply a return to grand theory or, *a fortiori*, to totalizing conceptions of society, but it does imply an empiricism of sorts, namely, a willingness to examine closely how these practices developed, what their effects are, and how their benefits and burdens are parsed among different individuals and groups in society. In this regard, the recurring theme of powerlessness, along with the corresponding admission that we "have no clear idea of what to work for" (Rorty 1989a, 182), should not be construed as being out of tune with the more celebratory temper of Rorty's narrative. Rather, both motifs should be seen as natural consequences flowing from the type of narrative that Rorty constructs, one that—ironically—implicitly shares the theorist's and traditional philosopher's animus toward all forms of empiricism and detailed historical and sociological description.[10]

REDESCRIPTION AND POLITICS

Let us turn now to Rorty's postmetaphysical vision of liberalism. Rorty's redescription of classical liberal thought proceeds, as we have seen, through a nonrationalist, nonuniversalist redescription of the hopes and aims of a liberal society. The key maneuver in this redescription is his recommendation that philosophers and political theorists drop their efforts to fuse theoretically the impulses for self-creation and justice. Instead, they should be content to recognize the import and legitimacy of both impulses, while carefully circumscribing the domain in which each is allowed to flourish. By insisting on a firm distinction between the public and private spheres, Rorty contends that hopelessly irreconcilable philosophical disputes can be transformed into concrete practical questions about how best to balance the incommensurable demands for self-creation and human solidarity. Striking adequate compromises between these two demands is, Rorty inveighs, the most important practical issue in liberal societies, but the skills required to negotiate such compromises are quite different from those of the theorist or traditional philosopher.

In examining this redescription, we might begin by posing the sort of instrumental, pragmatist questions that Rorty himself encourages us to ask: What is the practical benefit of this redescription? Does it illuminate or help us to resolve concrete social or political questions? Does it, in the spirit of Dewey, help us to determine or usefully consider which social practices we must preserve, cultivate, or extend and which we must refashion or jettison—especially, once we have dispensed with the hope of solving these questions through the application of neutral, context-independent criteria or theories? Here Rorty's pragmatic liberalism is at its weakest. For although his claims about the impossibility of philosophically or theoretically synthesizing "Nietzsche with Marx or Heidegger with Habermas" may be convincing (1989a, xiv), it is difficult to see how Rorty's description of a liberal utopia could operate as a starting point or provocative heuristic device for coming to grips with current social and political dilemmas. For instance, Rorty's insistence on a firm distinction between the public and private spheres may usefully remind us of the obvious dangers of eradicating the distinction altogether, but it does nothing to answer the truly difficult question of how that distinction is to be negotiated in practice. In current political debates about abortion rights, prayer in the school, family leave, governmental regulation of the economy, welfare payments to the poor, domestic violence, and so on, the issue under dispute isn't whether some distinction should be drawn between those aspects of life that are immune from governmental regulation and those which are not (this is instead the common starting point of the debate) but where, how, and on what basis that distinction is to be drawn.

Indeed, if we examine current ideological conflicts in the United States, what is striking is the way in which conservatives and liberals seek to defend diametrically opposed agendas through appeals to Rorty-like distinctions between the public and private spheres.[11] Broadly speaking, conservatives view markets and economic affairs generally as the realm of freedom, autonomy, innovation, and self-creation. They accept economic inequality as an unfortunate but inevitable effect of free markets, arguing that whatever cruelties the market may inflict on individuals, such cruelties are still less severe than those that would result from governmental efforts to regulate economic affairs and redistribute wealth. On the other hand, they argue that cultural, moral, educational, and lifestyle issues are issues of public concern; for without a consensus on basic moral and cultural questions, civility, decency, and public life in general cannot be sustained. Conversely, liberals contend that these same cultural, moral, educational and lifestyle issues belong in the private sphere, the sphere of negative freedom, autonomy, irony, and self-creation. They maintain that questions of moral belief, lifestyle choice, and the like are quintessentially private questions and thus should remain free from state control, regulation, and coercion. By contrast, the demands of justice and the obligation to minimize cruelty requires more control over economic affairs and the distribution of wealth than an unregulated market provides. Moreover, they argue that civility, decency, and civic-mindedness are best preserved not through the institution of moral and cultural orthodoxy but by ensuring humane living conditions and attenuating extreme economic inequalities.

This brief sketch of current political conflict illustrates the severe practical limits of Rorty's redescription of classical liberalism. Not only does his proposed separation of the public and private spheres fail to provide a clear alternative to or exit from current ideological combat, but it fails even to be a redescription in the strict sense of the word. As we have seen, both conservatives and liberals already appeal to distinctions identical to Rorty's own, yet they make these appeals in defense of diametrically opposed social and political policies. Hence, Rorty's distinctions between autonomy and justice, self-creation and human solidarity, private narcissism and public pragmatism are better understood as restatements of positions that both conservatives and liberals endorse, the difference being that they interpret these distinctions in very different ways.

Rorty would surely reply that only someone still under the spell of metaphysics and theology *could* demand something more than the narrative that he provides, for only such a person believes that there are things like a special method or general a priori principles that determine what counts as rational or moral action in particular circumstances. As a pragmatist, however, he cannot—and need not—appeal to any neutral, context-independent principles or criteria for determining the outcome of specific conflicts. At most, he can recommend a possible locus of public deliberation while supplementing this recommendation with a narrative describing its potential benefits and the potential dangers of competing alternatives. In politics, as in other areas of inquiry, the loyal pragmatist construes "rational" or "just" policies as those which emerge as a result of "undistorted communication" or "free and open encounters of present linguistic practices and other practices with suggestions for new practices" (Rorty 1989a, 60, 67). The actual resolution of particular conflicts can thus occur only through free-and-open public deliberations, not through the development of ahistorical theories of morality, politics, or justice.

This response might be credible if Rorty either convincingly demonstrated that "undistorted communication" or "free-and-open encounters" were indeed accurate descriptions of current political discourse in America or if he offered detailed analyses of current impediments to such encounters, along with proposals for curbing or remedying them. But Rorty rejects both of these options. He concedes openly that for all of American liberalism's virtues, there is still a clear and undeniable gulf between the ideal of free-and-open encounters and current political realities. Yet he also fails to examine those social practices and institutions that inhibit the development of a more free, open, inclusive, and democratic public discourse. Instead, he offers vague suggestions that circumvent entirely the difficult practical issues. For example, he says that "discoveries about who is being made to suffer can be left to the workings of a free press, free universities, and enlightened public opinion" and that "the only way to avoid perpetuating cruelty within social institutions is by maximizing the quality of education, freedom of the press, educational opportunity, opportunities to exert political influence, and the like" (1989a, 63, 66–67). Such statements may, as far as they go, be true, but they elide all the troublesome questions about the particular measures required to realize these abstract aims. Can one, for instance, seriously hope to maximize the quality of education and educational opportunities without improving teacher-to-student ratios, without paying teachers salaries comparable to those in other respected professions, without increasing the number of days a year that children are sent to school and without providing the necessary equipment for instruction?[12] And if not, how does one finance these reforms in an age of fiscal crisis and increasing voter distrust of government generally? In short, Rorty's utopian dream of "building new and magnificently equipped schools in the inner cities" may be an honorable one (p. 7); but this only raises further and considerably more contentious questions about the types of political, economic, cultural, and institutional transformations that might be required to make this utopia a reality and about the forms of collective action that are likely to be the most effective instruments of such changes.

These, of course, are only a few of the many issues that arise as soon as one moves from the level of abstract sentiments to the terrain of social and political action, yet Rorty's narrative repeatedly stops short of any serious or systematic exploration of them. In place of a detailed interpretation of the social context and genesis of problems like the "crisis in education," Rorty offers sweeping and unsupported generalizations about "an increasingly greedy and heartless American middle-class" that lets "the quality of education a child receives become proportional to the assessed value of its' parents real estate"[13] or that "is unwilling to pay the taxes necessary to give poor blacks a decent education and a chance in life" (1989b, 7; idem 1990b, 642). Such claims are themselves deeply contentious, yet even if one grants that middle-class greed and racism are indeed part of the problem, this too would seem to require further analysis and interpretation: Is the American middle class as fully unitary as Rorty assumes, or is it a complex collection of different social groups and communities (white, African-American, Asian-American, urban and rural, northern and southern, male and female, white-collar and blue-collar, Christian, Jew, and Muslim) with disparate and frequently conflicting interests, sensibilities, and social sympathies? Was it just a matter of "chance" or "accident" that this class, or segments of this class, became increasingly greedy, selfish, and racist during the past 15 years? Was this a matter of "choice"? And if not, what are the social, political, economic, institutional, and historical sources of this shift in social sensibilities? Unfortunately, Rorty never pursues these questions. Instead, he quickly closes off the conversation by declaring that "it seems to me a fact [that] we need no fancier theoretical notions than 'greed', 'selfishness', and 'racial prejudice' to explain these phenomena. . . . When I am told that to appreciate the significance of these facts I need a deeper understanding of, for example, the discourses of power of late capitalism, I am incredulous" (1990b, 642).

Here again, Rorty's legitimate suggestion (that there is a point at which further analysis of pressing social problems can become an evasion of, or substitute for, rather than a supplement to, political action) obscures the larger question of whether his remarkably vague proposals, along with his global claims about the causes of current social dilemmas, are accurate, compelling, or pragmatically useful (in Rorty's own sense of identifying a "pressure point for initiating change" [1990b, 643]). As I have argued, Rorty's recommendations not only fail to help identify pressure points for change, they represent a flight from that very task. In this respect, his original Deweyan ambition of transforming philosophy into an instrument of moral and political change—of bringing it back into "the conversation of mankind"—is abrogated by his own reluctance and inability to explore precisely those questions which are morally and politically urgent. Oddly, Rorty's preoccupation with avoiding a return to foundationalism, metaphysics, and Truth have left him so resistant to all forms of systematic analysis that his alternative to foundationalism is in many ways every bit as aloof from the complexities of ongoing social and political issues as the foundationalist philosophy which he sought to replace.

CONCLUSION

To return to the questions I posed at the outset, I hope to have shown that "redescription," as well as the broader Deweyan aim of reinvigorating social science and philosophy, must involve something more than just overcoming foundationalism or theoretical hegemony. It also means creating a space for the development of detailed portraits of our institutions and practices, of how they develop, of the effects they produce, and of the issues to which they give form. In addition, this means recognizing that the absence of an Archimedean point implies that issues of power are always potentially at stake in constituting, sustaining, or transforming our social practices. Rorty's own work is a testament to the fact that one can all too easily fuse sophisticated views about epistemology and philosophical foundationalism with political positions that are, however well intentioned, at best misconceived and at times self-stultifying. If we hope to avoid these problems, perhaps the approach to take would be to combine postmetaphysical insights on the status of knowledge not simply with "thin," global stories about the past but with detailed histories of practices and power.

Notes

I would like to thank Richard Ashcraft, Carole Pateman, Lyle Massey, and Bert Dreyfus for helpful comments on an earlier version of this essay.

1. Rorty describes his desired polity as both "postmodernist bourgeois liberalism" (1991a, 197–202) and, more recently, as a "liberal utopia." In a recent essay, however, he expresses regrets about his use of the term *postmodern* (which, following Jean-François Lyotard, he defines as "distrust of metanarratives"), primarily because it is now "so over-used that it is causing more trouble than it is worth" (1991b, 1).

2. Using case studies from the history of science, I have offered such an argument (Topper 1994, chap. 1). It should be noted that if (pace Rorty) it is sometimes possible to argue for or against the introduction of a new vocabulary, then the crucial question becomes one of deciding when, in the course of an ongoing exchange, it may become unreasonable or absurd to continue arguing. As Jacques Bouveresse has rightly stated: "Il n'est pas contestable qu'il arrive en philosophie, comme d'ailleurs également dans n'importe quelle autre entreprise intellectuelle, un moment où il est déraisonnable et absurd de vouloir continuer à argumenter. . . . Mais la difficulté est, comme toujours, de reconnaître le point où l'on doit s'arrêter" (1992, 47).

3. As Rorty explains, humiliation is a particularly devastating form of nonphysical cruelty, one that is tied closely to the ironist's preoccupation with redescription. Here, too, Rorty borrows from Judith Shklar's (1984) discussion in *Ordinary Vices*. For his account of humiliation and especially his reasons for thinking that irony and redescription must be kept private (in part because they have the potential of humiliating others), see Rorty 1989a, 90.

4. Although Rorty himself never appeals to such arguments, nonfoundationalists have also sought to answer the charge of relativism by pointing out that today many of the tenets of liberal democracy are almost universally accepted as principles of political legitimation, even if they are violated frequently in practice. This, they claim, makes the charge of radical relativism moot as a practical issue. See, e.g., Taylor 1992, 38.

5. The first three chapters of *Contingency, Irony, and Solidarity* are "The Contingency of Language," "The Contingency of Selfhood," and "The Contingency of a Liberal Community."

6. For an even more explicit link between contingency and "chance events," see the "possible world" sketch of Heidegger's life in Rorty 1990a. It should be emphasized that there is an important difference between asserting, for example, that there was "no particular reason why this ocular metaphor seized the imagination of the founders of Western thought" (Rorty 1979, 38) and simply declining to offer any reason(s).

7. This dichotomy between voluntarism and determinism has a corollary in Rorty's attempt to defend (1) the physicalist idea that "one day we will be able, 'in principle', to predict every movement of a person's body . . . by reference to the microstructures within his body" and (2) the existentialist commitment to the idea of radical freedom (1979, 354, 376–79). Interestingly, Rorty's effort to preserve both freedom and determinism by claiming that these are different descriptions of a single event or bodily movement is remarkably similar to Kant's attempt to reconcile moral freedom and natural necessity by viewing them as both true under different aspects of reality. See Bhaskar 1989, 164–65.

8. For a relevant discussion of this issue from the standpoint of art history, see Parker and Pollock 1981. In political theory, writers such as Carole Pateman have appropriately asked, "Why is Paine's reply to Burke's polemic against the French revolution studied, but not Mary Wollestonecraft's earlier reply? Why have the early socialists, who were concerned with relations between the sexes and new modes of household organization, been dismissed as 'utopian'? Why, more generally, are none of the feminist theorists' writing from the seventeenth century onward discussed, when the most minor male figures are given their due?" (Pateman 1987, 2–3).

9. It should be emphasized that Rorty's intent is not to preserve patriarchical or sexist social relations or modes of behavior. On his account, cruelty is by definition a public affair, whether it occurs in the home, workplace, or anywhere else. Nevertheless, his failure to examine the ways in which the public/private distinction has operated historically as a

bulwark against not only against sexual (and hence civic) equality but also against gays, lesbians, and other minorities raises the question of whether his version of the distinction (which in its formal features is almost identical to the classical distinction) is the most useful tool for advancing the ideals of liberal democracy. Given the historical record, it would seem incumbent on Rorty to show how his construal of the distinction would operate in practice to reduce sexual and gender inequalities and to explain why this construal would be more useful than the alternative versions proposed by recent feminist writers. Compare Will Kymlicka's (1991) recent attempt to apply an interpretation of liberalism to current conflicts regarding the rights of minority cultures.

10. In recent writings, Cornel West (1993a, 1993b) makes similar criticisms of Rorty. West contends, for example, that Rorty embraces "an aestheticized version of historicism in which the provisional and variable are celebrated at the expense of highlighting who gains, loses or bears what costs" and that he offers "'thin' historical narratives which rarely dip into the complex world of politics and culture" (1993a, 23, 127; see also West 1993b, 177). While the preceding analysis strongly underscores these remarks, I would perhaps differ with West on two issues. First, West implies that Rorty's errors are chiefly (contingent) ones of omission, that is, he neglects thick, politically engaged historical description and fails to articulate the social and political implications of specific social practices and institutional arrangements (for hints of stronger objections, see West 1993b, 177). I argue, however, that Rorty not only fails to engage in detailed social, historical and institutional analysis but lacks the conceptual resources for such analysis. At a minimum, such an undertaking would require, if not a *theory* in Rorty's sense, at least a satisfactory (or prudent and plausible) account or (re)description of power and power relations, something Rorty (like the classical pragmatists) not only omits but cannot develop without also revising considerably his understanding of contingency. Second, West, like other critics, tends to accent the celebratory aspects of Rorty's narrative, criticizing it for its inattentiveness to the everyday miseries and cruelties that punctuate the lives of so many Americans. I would maintain, however, that such criticisms are incomplete, for they overlook the fact that Rorty's narratives contain both an effusive optimism *and* a deep and often fatalistic pessimism. Indeed, I take it that any adequate assessment of Rorty's political thought must account for the disconcerting shifts between these binary poles.

11. My sketch of ideological conflict is indebted to Jeff King's discussion (1990, 8–9). While King's analysis accents "the problem of the mutual intrusion or colonization of the public and private spheres" (p. 7), my concern is to show the practical limitations of Rorty's redescriptive efforts, as well as his conceptual inability to deal with concrete social problems.

12. For some recent efforts to discuss difficult and controversial issues of educational reform, see Gutmann 1987 and Barber 1992.

13. The claim that current social ills in the United States are due primarily to an increasingly greedy middle class is a recurring theme in Rorty's recent work. See Rorty 1990b, 642; idem 1991a, 15, n. 29.

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RESEARCH NOTE

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WINNING THE LOCAL VOTE: THE EFFECTIVENESS OF CONSTITUENCY CAMPAIGN SPENDING IN GREAT BRITAIN, 1983–1992

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Much recent analysis of British politics has assumed, explicitly or implicitly, that constituency campaigns have no impact upon an electorate that draws on an increasingly nationalized media for its information. We employ data on constituency campaign spending to challenge this interpretation. Local party campaigners are rational in their use of funds, spending most in seats where the competition is close and least where there is little hope of winning. What is more, campaign spending is clearly associated with voting, increasing support for the spending party and decreasing support for its rivals. Contrary to the accepted wisdom, local campaign spending can result in important shifts in the vote. However, local campaigning seems to be of much more value to challengers than to incumbents.

Electoral campaigning is a major part of any political party's activities. Campaigning fulfils a multiplicity of functions. Ideally, it should inform the electorate about a party's policy, program, and ideology. Furthermore, campaigning can serve as a vehicle for generating commitment and identity among party workers. Most importantly, campaigning aims to affect election results. It plays a part in marshaling votes by providing information that should both encourage people to ensure that they exercise their franchise and convince them to vote for the party that is soliciting their support: if effectively carried out, it should ensure both that a party's supporters turn out to vote and that new converts are won over.

Mounting an electoral campaign costs money, however. The costs of making handbills, posters, billboards, and election broadcasts all must be met. Not surprisingly, then, funding electoral campaigns is one of the major uses of party finance.

In the British literature on this topic, attention has been focused on spending on national political campaigns (Pinto-Duschinsky 1981, 1985, 1989). This is to be expected, given the very large amounts spent by the major parties on national electioneering. Nevertheless, a full evaluation of party expenditure requires work on the individual constituency campaigns as well, and that is the focus of the analyses here.

Elections are won or lost not just in the country as a whole, but also in each of the parliamentary constituencies (of which there were 651 in the United Kingdom at the 1992 general election, although our analyses focus on the 633 in Great Britain only.)[1] Constituency campaigns, while not as dramatic or as widely reported as their national counterparts, also aim to improve voter turnout. Those campaign costs fall predominantly on local constituency parties, not on the national machines, so that any consideration of the effectiveness of party finance must take into account constituency campaign spending. We shall examine the pattern of local party spending on constituency electioneering in Great Britain and its impact on the election results.

LOCAL CAMPAIGNS IN AMERICAN ELECTIONS

North American research on campaign spending does not concentrate solely on the national picture (although see Jamieson 1992). Much work has been carried out on several aspects of local campaign finance in congressional and gubernatorial contests. Some researchers have investigated how campaign finances are raised, with recent work focusing on both the role of national party decision making in the distribution of campaign resources (Herrnson 1989) and on the importance of "friends-and-neighbors" factors in raising funds locally (Thielemann 1993).

However, of more direct relevance to the work reported here is research on the efficacy of local campaign effort. Once again, the North American literature contains numerous examples of work in this vein. Much of that work has concentrated on the impact of local campaigning on voter mobilization, showing that active campaigns are more effective at getting the vote out than are moribund campaigns (Caldeira et al. 1990; Huckfeldt and Sprague 1992; Patterson and Caldeira 1983).

The ultimate test of campaign efficacy is the effect of campaigning on the actual vote. Numerous studies of American politics have pointed to a link between the amount spent by a local candidate on election campaigning and the candidate's electoral support. However, the conclusions are not clear-cut. One influential study of congressional elections claims that "spending by *challengers* has a substantial impact on election outcomes, whereas spending by *incumbents* has relatively little effect. . . . The more incumbents spend, the worse they do" (Jacobson 1978, 469). This, he argued, was a consequence of the imbalance between challengers and incumbents. Because they enjoyed the benefits of free publicity by virtue of their office, incumbents did not need to spend heavily on their reelection campaigns. Only when they were seriously threatened by strong—and well-financed—challenger campaigns did they have to spend reactively. High incumbent spending was, therefore, associated with unfavorable contests, hence the counterintuitive relationship between high spending and poor results. Jacobson's conclusions have been challenged in some recent work, which suggested that "incumbents are able to defend their seats quite effectively through campaign spending" (Green and Krasno 1988; but see Green and Krasno, 1990; Jacobson 1990). Whatever the outcome of this controversy, however, it is clear that local campaign spending in American elections is associated, at least for some candidates, with improved performance at the ballot box.

LOCAL CAMPAIGNS IN BRITISH ELECTIONS

In contrast, there has been rather little interest in the efficacy of local campaign effort at British elections. The accepted wisdom has tended to be that because of the relative importance of party over candidate in the British system and because of the rising importance of the national mass media as vehicles for political reporting and for campaigning, the constituency campaign has declined to something of ritual importance only and has "little success in changing political attitudes" (Kavanagh 1970, 87). Therefore, much more attention has been given to the national campaign than to the local (Crewe and Harrop 1986, 1989).

That said, there are a few studies that do run against this accepted view. The focus is often a single constituency (e.g., Holt and Turner 1968). The main conclusion of such studies is that other things being equal, extra campaign effort and, especially, more thorough and more frequent canvassing of electors, can pay dividends in mobilizing the vote (Bochel and Denver 1971, 1972).

But few studies look at campaign effort across large numbers of constituencies. One exception is based on research on Labour party members (Seyd and Whiteley 1992). That study uses party members' self-reported levels of campaign activity to estimate local levels of constituency effort and shows that the more active a constituency Labour party was, the better it did at elections. However, that study was limited to only 191 constituencies (out of the 633 in Great Britain) and to only one party. To appreciate the role of constituency campaigning fully, it is necessary to go beyond this. A priori, an effective campaign would be expected not only to push up a party's vote, but also to lower the votes of its rivals. Local campaigning takes place in a dynamic, multiparty context, and we need to take that context into account.

The difficulty here is that there is little consistent information available on campaigning by all parties in all seats. One of the few indicators available that might go some way to filling the gap is the amount spent on the constituency campaign. By law, this information must be given to the local returning officer (an official in charge of the conduct of an election locally) after each election and it is published by the government.

Some studies have employed these data to evaluate campaign effectiveness (Denver and Hands 1974, 1985; Johnston 1979a, 1979b, 1985a, 1985b, 1986, 1987; Johnston, Pattie, and Johnston 1989). Doubts have been raised over the reliability of spending as a guide to campaign effort (Gordon and Whiteley 1980; but see also Johnston 1981). Critics have argued that the reported figures are unreliable in themselves and that since much constituency campaign activity is voluntary and free, spending data miss out the most important aspects of the campaign. However, more recent work, comparing spending data with local activists' self-reported campaign activity (derived from Seyd and Whiteley's Labour party study) and with constituents' recollections of being canvassed (taken from the 1987 British Election Study) clearly demonstrates that constituency spending is a good surrogate for campaign effort. Local campaign spending, activists' self-reported assessments, and voters' recollections of being canvassed are all highly intercorrelated: the more a local Labour party spent on its constituency campaign, the more likely its members were to report a hard-fought contest and the more likely local voters were to report being canvassed on behalf of the Labour party (Pattie et al. 1994; for further support, see also Denver and Hands 1993, 234). We are confident, therefore, that constituency campaign spending is measuring the strength of the local campaign.

In the following analyses, we employ the spending data to investigate the role of constituency campaigning in the 1983, 1987, and 1992 British general elections: these are the most recent general elections, and all three were fought in the same constituency boundaries, making cross-election comparisons much easier. Because of the very different party system operating there, we exclude Northern Ireland from the analysis and concentrate on the 633 constituencies of Scotland, England, and Wales. We are concerned with three issues: (1) whether local campaigners are "rational" in their allocation of effort, (2) whether local campaigning makes a real difference to election outcomes, and (3) whether the local campaign has different implications for incumbents and challengers.

LEGAL CONSTRAINTS ON LOCAL CAMPAIGN SPENDING

The law covering campaign finance at British general elections differs in a number of key respects from the

TABLE 1

Constituency Campaign Spending by Party, 1983–1992 (Summary Statistics)

	PARTY SPENDING ON CONSTITUENCY CAMPAIGNS (N = 633)							
	CONSERVATIVE		LABOUR		LIBERAL		NATIONALIST	
SPENDING	MEAN	S.D.	MEAN	S.D.	MEAN	S.D.	MEAN	S.D.
In £								
1983	3,342	1,094	2,947	1,008	2,525	1,224	1,668	1,118
1987	4,441	1,407	3,945	1,296	3,434	1,692	1,820	1,424
1992	5,787	1,861	5,055	1,730	3,218	2,428	2,517	1,833
As % of Legal Maximum								
1983	74.06	22.41	66.53	23.78	56.23	26.86	39.09	25.72
1987	78.26	22.74	70.87	24.31	60.49	28.83	33.82	26.01
1992	79.22	22.92	70.63	24.81	43.49	31.85	36.49	25.97

Note: *Liberal* is used here as shorthand for the center party. In 1983 and 1987, this was the Liberal-Social Democratic Party Alliance; in 1992, it was the Liberal Democrats. The figures for *nationalist* campaign spending refer to both Plaid Cymru (the Welsh nationalist party) and the Scottish National Party. The figures refer to Scottish and Welsh seats only.

Sources: Home Office, *General Election Expenses, 1983* (London: HMSO, 1983); idem, *General Election Expenses, 1987* (London: HMSO, 1988); idem, *General Election Expenses, 1992* (London: HMSO, 1993).

situation in the United States (for details, see Herrnson 1993; Jones 1993; of Johnston and Pattie 1993a). Therefore, we shall outline the rules governing constituency campaign spending in Britain.

There is a marked difference in the legal status of local and national campaign spending in British elections. There are few controls on how much a political party can raise and spend on a campaign at the national level, so long as no individual constituencies are mentioned in the campaign material. The major constraint is the size of the party's election war chest. At the local level, on the other hand, maximum spending limits are defined by law; and after an election, candidates' agents must give full details of their campaign expenditure to the local electoral returning officer. If a party spends above the legal limit in the constituency or if there are doubts over the accuracy of its accounts, legal action can be initiated.

The discrepancy in the legal standing of national and local campaign spending is produced by still extant nineteenth-century legislation designed to remove the possibility of corrupt practices in electioneering (O'Leary 1962).[2] Those laws were intended for a political environment in which the main thrust of campaigning took place at the constituency level: their development has not kept pace with the rapid post-1945 increase in the relative importance of national campaigning through the major newspapers and the electronic media.

The law currently controls all expenses incurred by candidates before, during, and after an election as part of their campaign, by setting maximum spending levels. This excludes personal expenses (money spent on travel, accommodation, etc.) but includes the fees of election agents and clerical workers, printing and stationery costs, the cost of public meetings, and so on. The legal maximum for election expenses varies from constituency to constituency, reflecting the geographical size of the constituency and the size of the local electorate. The main division is between urban (or "borough") and rural (or "country") seats. Because they generally cover a larger geographical area than urban seats, rural constituencies are held to be more expensive, elector for elector, to campaign in. As a consequence, the legal limits on campaign spending in rural seats are slightly more lenient than in urban ones. At the 1992 election, the statutory maximum expenditure allowed was, for county (i.e., more rural) constituencies, £4,330 plus an additional 4.9 pence for every entry on the electoral register, and for borough (i.e., largely urban) constituencies, £4,330 plus an additional 3.7 pence for every entry on the electoral register.

THE VOLUME OF CONSTITUENCY CAMPAIGN SPENDING

There are wide variations in the level of local campaign spending, both within and between parties. In general, local Conservative parties spend more on their constituency campaigns than do local Labour parties: both usually outspend the Liberals and the nationalists (top of Table 1).[3] In 1983, for instance, the average Conservative constituency association spent £3,342 on its election campaign there, compared to £2,947 for Labour, £2,525 for the Liberal–SDP Alliance, and only £1,668 for the nationalist parties: by 1987, the relevant average spending levels were £4,441, £3,945, £3,434, and £1,820 respectively, reflecting both the upward movement of the legal maximum and inflation in election expenses over the four years. Finally, in 1992 spending by the Conservative and Labour candidates was substantially more than it had been five years previously, whereas the average Liberal Democrat candidate spent less than an Alliance counterpart did in 1987.

Part of the variation between constituencies in the volume of campaign spending by the parties reflects differences in the legal maxima arising from differences between county and borough constituencies, differences in the size of electorates, and the effects of price inflation between elections. We can control for these effects by expressing the amount spent by a party in a constituency as a percentage of the legal maximum there. (As we have already seen, the maximum depends on whether the seat is urban or rural and on the size of the local electorate: the maxima are also regularly adjusted to take inflation into account.) This reveals the differences between parties even more starkly (bottom of Table 1).

The average constituency party spent well below the maximum allowed in the average constituency at each election. Conservative candidates spent closer to the limit than did either constituency Labour parties or Liberal parties: in 1983, on average, Conservatives spent about 75% of the money they were entitled to, compared to 66% for Labour, 56% for the Alliance, and 39% for the nationalists. The 1987 election saw local Conservative, Labour, and Alliance parties move closer to the limit (Conservatives 78%, Labour 71%, Alliance 60%) while the nationalists fell back in relative spending (the only major party bloc to do so), at 34%. Finally, in 1992, Conservative spending remained almost exactly the same, but spending by the Liberal Democrat party fell by some 17 percentage points on average. If local spending is important, then the Liberal Democrats were substantially disadvantaged in 1992.

The spread of spending levels among constituency parties also changed over the period. The standard deviations reported in the top of Table 1 increased consistently over time, indicating that party spending in 1992 varied more around the average than in 1983. This was most marked in the case of center party spending: the standard deviation for Alliance spending in 1983 was £1,224, but it had doubled to £2,428 by 1992. The financial gulf between high spending and low spending constituency parties became wider during the 1980s and early 1990s.

In part, the increasing variability among constituencies in party spending was a consequence of inflation in the costs of local campaigning and of increases in the legal limits between 1983 and 1992. The equivalent standard deviations for Labour and Conservative spending as a percentage of the legal maximum show few important changes between elections in the spread of spending (bottom of Table 1). The picture is somewhat different in the case of Liberal spending: over time, center party spending became more variable not only in absolute but also in relative terms. At the grassroots level, the Liberals are not as well organized throughout the country as Labour and the Conservatives: their strength has been concentrated in a relatively few constituencies. To some extent, the apparent increase in the differential ability of local Liberal parties to spend on election campaigns may be a reflection of that geography of party strength: the Liberals may have reaped the benefits of active memberships in their strongholds, in the form of extra resources and greater incentives to spend. Meanwhile, in seats where they are not so strong, the opposite may hold, such that low membership and few activists means limited resources and little incentive to spend on hopeless contests. Furthermore, between 1987 and 1992, the Liberal–SDP Alliance was hit by an acrimonious debate over a proposal for a formal merger (from which the Liberal Democrats eventually emerged). One consequence of the merger was that a large minority of members left the party, some to form breakaway Liberal and SDP parties and some dropping out of center party politics altogether. The consequences for Liberal Democrat finances, particularly at the local level, was undoubtedly reflected in the party's money-raising capacity.

To a large degree, the spending patterns just described match the differences between parties in their national spending at general elections. (At the 1992 general election, for instance, the Conservatives claimed to have spent £10.1 million, compared to Labour's £7.1 million and the £2.1 million of the Liberal Democrats [see Butler and Kavanagh 1992, 260].) There is a clear hierarchy in the resources available to political parties at both national and local levels. Evidence on the fundraising capacities of local political parties is hard to come by, but it is unlikely that all have equal access to resources: some will be better placed to pay for campaigns than will others. In general, local Conservative associations seem to be best placed in terms of local fundraising.

THE EFFICACY OF CONSTITUENCY CAMPAIGN SPENDING

The fundamental issue, however, is not whether parties spend more or less "sensibly" on their local campaigns but whether this spending has any relationship to actual electoral outcomes. Mounting a strong local campaign may work wonders for party morale in a constituency, but if local campaign spending has no effect upon local voting, then it is money (and the effort involved in raising it) wasted, no matter how well targeted. (That constituency campaign effort and spending have little, if any, impact on election results is a widely accepted conclusion among British psephologists, usually without any detailed evidential backing [see Kavanagh 1970]. This view is also put forward in the well-regarded Nuffield election studies [see e.g., Butler and Kavanagh 1984, 267; idem 1992, 245; Butler and Pinto-Duschinsky 1971, 334].) We turn, therefore, to a consideration of the electoral effects of constituency campaign spending.

If local campaign spending has an effect upon local election results, we would expect a direct relationship between the amount a party spent and the number of votes cast for it. The more a party spends, the more votes we expect it to win. Conversely, however, the more effort a party's rivals put in to the local context, the more they should benefit in terms of votes and the more damage that would do to the other party.

Thus, we anticipate a positive correlation between a party's spending in a constituency and its vote share there and a negative correlation between that party's vote share and the amounts spent by its rivals.

To test this hypothesis, we have fitted a series of regression models, with party vote share as the dependent variable and spending levels as the independents: the goal is to identify the extent to which the pattern of voting for a party across the various constituencies is predictable from the pattern of party campaign spending. An important objective of the local campaign is to win over people who have previously supported another party. While winning converts is an important part of the local campaign, however, it is probably not the most important aspect in British election campaigns (Bochel and Denver 1971). Of greater importance is the process of identifying *existing* party supporters and making sure they actually turn out to vote on election day. This means that a large part of the campaign effort will be directed at trying to mobilize and win votes from those supporters who have either just joined the electorate or who did not vote at the previous election. To take this into account, we calculate vote share as a percentage of the local electorate. Furthermore, since the total level of local campaign spending is determined not only by the resources available to and the strategic judgments of local campaigners, but also by the legally allowed maximum, we need to take into account the "ceilings" within which local campaigns are fought: not to do so would risk confusing the effects of strong local campaigning with the extra costs of fighting an election in a large constituency. In the regression models, we therefore express a party's spending in a constituency as a percentage of the legal maximum there.

For each of the last three general elections, we estimate three equations, one each for the Conservative, Labour, and Liberal–SDP Alliance (Liberal Democrat in 1992) shares of the electorate. (We have excluded the Scottish and Welsh nationalist parties from the analysis: neither party contests every seat in Great Britain, and both—but more especially the Welsh nationalists—base part of their appeal on cultural as much as political grounds [Agnew 1987; Johnston and Pattie 1989].) Each equation, however, contains figures for the spending levels of all three major British parties, because we expect high spending by rival parties to offset high spending by the party under investigation. The spending variables are

CSnPC = Conservative spending in a constituency at election n as a percentage of the legal maximum there

LSnPC = Labour spending at election n in a constituency as a percentage of the legal maximum there

ASnPC = Alliance spending at election n in a constituency as a percentage of the legal maximum there.

Therefore, we expect a positive relationship between Conservative spending and the Conservative vote share but a negative relationship between that party's share and both Labour and Alliance (Liberal Democrat) spending.

Local campaign spending is only one of the factors likely to affect parties' vote shares at an election, as many studies of the geography of voting have shown. Constituency election results are also influenced by local characteristics such as the class structure, the pattern of housing tenure, the industrial base, and so on (Butler and Stokes 1974; Crewe and Payne 1976; Dunleavy 1979; Heath et al. 1991). Furthermore, and most markedly during the 1980s, there have also been clear geographical disparities in electoral behavior, with voting trends moving in separate directions in different regions (toward the Conservative party in the south and the more rural areas and toward Labour in the north, including Scotland and Wales, and in the urban areas). During the 1980s, this regional divide widened markedly. While it narrowed somewhat at the 1992 election, it remains a major feature of Britain's political landscape (Johnston, Pattie, and Allsopp 1988; Johnston and Pattie 1993b). Before we can properly assess the effectiveness of local campaign spending, therefore, we must take these other factors into account. To do this, we have introduced some control variables into the models.

Our models control for those aspects of the local social structure of constituencies that are known to be linked to the geography of party support in Britain and also for regional context. At the time of writing, data from the 1991 Census were not yet available for parliamentary constituencies, so the 1981 Census is used here to provide indicators of social structure. Four social structure variables have been employed:

SEMI/UNSKILLED. The proportion of the economically active population at the 1981 Census in semi and unskilled manual jobs;

MINING. The proportion of the economically active population at the 1981 Census employed in the energy industry (in large part miners);

COUNCIL. The proportion of households at the 1981 Census in properties rented from local government; and

NCWP. The proportion of people at the 1981 Census living in households headed by someone from the New Commonwealth or Pakistan.

Based on past research, we expect that the Conservative vote should decline, and the Labour vote rise, as a constituency becomes more working-class and has more people employed in the energy industry, a larger public housing sector, and more residents living in New Commonwealth– or Pakistan-headed households. Thus, there should be a positive relationship between Labour's share of the votes cast and SEMI/UNSKILLED, MINING, COUNCIL, and NCWP and a negative relationship between each of these and the Conservative share.

Further controls were introduced for regional context. To take this into account, we created 22 dummy variables, one for each of our regions (based on standard planning regions but with major conurbations differentiated from their hinterlands).[4] Each dummy variable was coded 1 if a constituency was in the relevant region and 0 if it was not. In the regression analyses, the Outer South East was excluded (because it was the region in which the Conservative party tended to do best, and Labour worst) and serves, therefore, as the comparator region. All results for the regional dummies are expressed as deviations from this region and show the degree to which constituencies vary from the average vote percentage for that party in a constituency in the Outer South East. A positive coefficient in the equation for Conservative voting, for example, shows a region with a higher share for the party than in the Outer South East, whereas a negative coefficient indicates a lower than average share.

Once the effects of these factors have been taken into account, we can get a clearer view of the effectiveness of constituency campaign spending. Because we have controlled for both the general geography of voting and for the overall geography of swings in party support between regions, we can concentrate more confidently upon the links between constituency campaign spending and constituency voting. All the above variables were entered into the regression models. The control variables are not included in the tables reported below, which concentrate solely on the campaign spending variables (full details of the regression models are available on request from the authors). The results are shown in Table 2. At all three elections, the expected relationships emerged between the control variables and vote share for the Conservatives and Labour. Labour did better, and negative coefficients show that the Conservatives did worse, in constituencies with high concentrations of semi- and unskilled workers, council tenants, energy workers, and people from New Commonwealth backgrounds. The Labour vote was also higher—and the Conservative vote lower—in the northern regions than in the Outer South East at each election.

The Alliance (Liberal Democrat) vote was much less predictable by the controls—not surprisingly, given that the center party vote is traditionally less firmly rooted in the social structure than is voting for the other two major parties, and that at both the 1983 and 1987 elections the Alliance did uniformly well throughout almost all of the country.

Our main interest here is in the performance of party spending as a predictor of vote share, once the other control variables have been taken into account. The results in Table 2 give clear evidence that local campaign effort was significantly related to constituency election results: the harder a party campaigned in a seat, the better it did and the worse its rivals did. What is more, the effect was as expected, with all of the significant coefficients having the expected sign.

TABLE 2

The Impact of Constituency Campaign Spending on Party Share of the Electorate

INDEPENDENT VARIABLES	PERCENTAGE SHARE OF THE ELECTORATE (N = 633) CONSERVATIVE	LABOUR	LIBERAL
Election year 1983			
Con spending 1983	.112	−.063	.014
	(.010)	(.011)	(.011)
Lab spending 1983	−.060	.127	−.050
	(.007)	(.008)	(.008)
Lib spending 1983	−.018	−.058	.105
	(.007)	(.007)	(.007)
R^2	.87	.86	.51
Election year 1987			
Con spending 1987	.116	−.071	−.000
	(.010)	(.010)	(.011)
Lab spending 1987	−.058	.158	−.058
	(.008)	(.008)	(.009)
Lib spending 1987	−.034	−.074	.141
	(.007)	(.008)	(.008)
R^2	.88	.89	.62
Election year 1992			
Con spending 1992	.152	−.082	.002
	(.012)	(.012)	(.011)
Lab spending 1992	−.066	.164	−.075
	(.009)	(.009)	(.008)
Lib spending 1992	−.045	−.108	.177
	(.008)	(.008)	(.007)
R^2	.86	.88	.75

Note: Data are unstandardized regression coefficients (OLS) controlling for % semi/unskilled manual workers, % workers in mining and energy, % council tenants, % with household head born in New Commonwealth and region. Standard errors are in parentheses. The table does not report the full regression equations, but is limited to the coefficients for spending: full details of the equations, including all control variables, are available on request.

Source: OPCS, *1981 Census Small Area Statistics* (London: HMSO, 1981).

To take one example, once the other factors had been taken into account, the Conservative share of the electorate in 1983 rose by .112 percentage points for every 1 percentage point increase in the proportion of the legal maximum spent by the local Tories on their constituency campaign. Given that the average Conservative constituency association spent 74% of the legal maximum on its campaign in 1983 (bottom of Table 1), this represents an average of 8.4 extra percentage points over what the party could have expected from the local social and regional context alone. However, local campaign spending by the two other main parties eroded Conservative support. Thus, for every percentage point more spent by Labour, the Conservatives lost the votes of .06 percentage points of the electorate, and they lost .018 percentage points for every extra percentage point spent by the Alliance. Nevertheless, overall, the Conservatives were more efficient at converting campaign spending into votes than were their opponents. The same conclusion applies to the other two parties at each election, except for Labour in 1992: this leads to the general, though not quite unchallenged, conclusion that the more a party spends relative to the maximum allowed, the better it does at the election, however much its opponents spend.

Similar patterns held for the Labour and Alliance parties: they gained extra votes from their own spending and lost support as a result of other parties' campaigning. It seems likely that this was due to the effect of campaigning on voter turnout, rather than in converting voters from one allegiance to another. If parties spend more, more of their supporters turn out: where their rivals spend more, more supporters of their opponents turn out. Therefore, party spending did play a marked part in the battle for votes: local campaigning was an effective use of party resources.

PARTY INCUMBENCY AND CAMPAIGN EFFECTIVENESS

The equations just reported assume that the same relationships hold in all constituencies throughout the country. However, this is not necessarily the case. One major factor that may alter the picture is the effect of incumbency. Sitting Members of Parliament (MPs) have inbuilt advantages over their challengers in that they enjoy the powers of office and the benefits of publicity throughout the life of a parliament and can also draw on their record of constituency and parliamentary work (Norris, Vallance, and Lovenduski 1992; Wood and Norton 1992; Pattie, Fieldhouse, and Johnston 1994). Since British MPs have less individual power and are more tightly tied to party than are their American congressional peers, incumbency advantage in the United Kingdom is less substantial than in the United States. Nevertheless, it does exist. We might, therefore, expect that campaign effort would have different effects for incumbents than for challengers.

As we have already seen, the influence of incumbency on the effectiveness of campaign spending has been addressed in the relevant North American literature. However, its impact has been little studied in British elections (but see Johnston 1985a, 1987).

We examine this factor, initially by repeating the regression analyses reported in Table 2, but running separate models for seats defended by Conservative and Labour MPs. Because our main interest is the effectiveness of local party campaign spending, not support for individual MPs, we concentrate here not on individual incumbents but on incumbent parties; that is, we look at seats held by each party at the previous election, whether both contests were fought by the same MP or not. (There were insufficient Alliance or nationalist incumbents to justify separate analysis of their seats. Analyses were also carried out with dummies for *individual* incumbents, but this proved to make no substantive difference to the conclusions.) One difference between these incumbency models and the analyses reported earlier lies in the use of the regional dummy. Because Labour was very unsuccessful in the Outer South East region during the 1980s, this region was not a satisfactory "control." Instead, we have removed the Rest of the West Midlands region from the analysis, because both major parties were well represented in the region and because, of all the regions, it most closely resembled the national average. The results of these analyses are reported in Tables 3 and 4. Once again, we have little to say about the control variables: they all behaved as expected and are not reported in the tables. Attention is focused on the campaign spending variables.

Constituency campaign spending by parties challenging the incumbent party in a seat had much the same impact as we might expect. The more that a challenger spent on the constituency campaign, the higher his/her own share of the vote and the lower the share won by his/her rivals. What is more (and again as expected), spending by a challenger tended to have a stronger beneficial effect on that party's

TABLE 3

The Impact of Constituency Campaign Spending on Party Share of the Electorate, Seats Defended by the Conservatives

INDEPENDENT VARIABLES	PERCENTAGE SHARE OF THE ELECTORATE		
	CONSERVATIVE	LABOUR	LIBERAL
Election year 1983 (N = 360)			
Con spending 1983	.008 (.014)	.006 (.016)	.013 (.019)
Lab spending 1983	−.067 (.008)	.136 (.009)	−.042 (.010)
Lib spending 1983	−.034 (.007)	−.043 (.008)	.090 (.009)
R^2	.71	.79	.46
Election year 1987 (n = 397)			
Con spending 1987	.008 (.015)	−.020 (.019)	.030 (.020)
Lab spending 1987	−.079 (.008)	.159 (.010)	−.038 (.010)
Lib spending 1987	−.037 (.006)	−.062 (.008)	.121 (.008)
R^2	.77	.84	.65
Election year 1992 (n = 376)			
Con spending 1992	−.003 (.022)	−.057 (.028)	.061 (.026)
Lab spending 1992	−.084 (.008)	.174 (.011)	−.086 (.010)
Lib spending 1992	−.040 (.007)	−.112 (.009)	.160 (.009)
R^2	.68	.85	.77

Note: Data are unstandardized regression coefficients (OLS), controlling for % semi/unskilled manual workers, % workers in mining and energy, % council tenants, % with household head born in New Commonwealth, and region. Standard errors are in parentheses. The table does not report the full regression equations but is limited to the coefficients for spending: full details of the equations, including all control variables, are available on request.

TABLE 4

The Impact of Constituency Campaign Spending on Party Share of the Electorate, Seats Defended by Labour

INDEPENDENT VARIABLES	PERCENTAGE SHARE OF THE ELECTORATE		
	CONSERVATIVE	LABOUR	LIBERAL
Election year 1983 (n = 260)			
Con spending 1983	.099	−.047	.002
	(.012)	(.013)	(.013)
Lab spending 1983	.006	.009	−.024
	(.013)	(.014)	(.014)
Lib spending 1983	−.004	−.060	.081
	(.009)	(.010)	(.010)
R^2	.78	.67	.44
Election year 1987 (n = 209)			
Con spending 1987	.072	−.047	.005
	(.012)	(.011)	(.011)
Lab spending 1987	.004	.001	.005
	(.016)	(.015)	(.015)
Lib spending 1987	−.047	−.044	.104
	(.013)	(.011)	(.012)
R^2	.74	.79	.46
Election year 1992 (n = 229)			
Con spending 1992	.098	−.043	−.009
	(.014)	(.012)	(.009)
Lab spending 1992	.012	−.028	.009
	(.018)	(.016)	(.012)
Lib spending 1992	−.065	−.044	.092
	(.014)	(.012)	(.009)
R^2	.68	.72	.50

Note: Data are unstandardized regression coefficients (OLS) controlling for % semi/unskilled manual workers, % workers in mining and energy, % council tenants, % with household head born in New Commonwealth, and region. Standard errors are in parentheses. The table does not report the full regression equations but is limited to the coefficients for spending: full details of the equations, including all control variables, are available on request.

own support than it had a detrimental effect on the vote for other parties. For instance, in seats defended by the Conservative party at the 1992 election, Labour won an extra .17 percentage points of the electorate for every 1-percentage point increase in its spending as a proportion of the constituency maximum. But the same increase in Labour spending in Conservative seats shaved only .08 percentage points off both the Conservative and the Liberal Democrat shares of the electorate (Table 3).

Perhaps the most striking finding here, however, is that spending by incumbent parties seems to have no significant impact either on their own levels of support or on the support for other parties. The only exception to this comes in the case of Conservative spending in Conservative-held seats at the 1992 election. But in this case, the coefficients only just reach significance, and only have an effect on the support of Labour and the Liberal Democrats, not on the Conservatives themselves. That said, while increasing Conservative spending in Conservative-held seats at that election was associated with decreasing support for Labour, it was also associated with *increasing* support for the Liberal Democrats, a somewhat counterintuitive finding. This may be related to tactical voting—that is, to supporters of third-placed parties casting their vote not for their first preference but for a "second-best" party, to keep an unwanted party from winning in the seat (Johnston and Pattie 1991; Niemi, Whitten, and Franklin 1992). So an intensive Conservative campaign may focus the minds of Labour supporters in seats where Labour has little chance of success, and galvanize them into supporting the Liberal Democrats rather than see the Conservatives win. Equally, however, given that much of the Conservative campaign in 1992 was devoted to attacking Labour as untrustworthy, this result might also be the outcome of a general anti-Labour vote encouraged by the Conservative campaign.

This exception aside, the general pattern is clear. Campaign effort by challenging parties seems to pay dividends, but similar effort by incumbents has little effect. This finding is repeated in both Conservative- and Labour-held seats and is in evidence at all three elections studied here. In part, this may reflect the sort of "weak incumbent" problem described by Jacobson in the context of American election campaigns. However, as we shall see later, incumbent parties in Britain tend to spend very close to their legal limits. The apparent lack of an incumbent spending effect here may be an "asymptote" effect, created by the rather small variability in incumbent spending levels, itself a product of the ceiling placed on local campaign spending by statute.

Simultaneity and Estimation

The ordinary least squares estimates we have reported may be unreliable, however. Spending by incumbent parties is unlikely to be independent of spending by challengers and *vice versa*. As Jacobson (1978) has pointed out for election spending in the United States, it is probable that incumbent parties will increase their local campaign spending when faced with a strong local challenge: in this scenario, high spending by an incumbent is a mark of weakness, not of strength. Because the official election period is shorter in Britain than in the United States and because of Britain's much tighter controls on local spending, there are limits to how responsive one party's local campaign can be to those being waged by others in the same seat, especially if an asymptote effect operates for incumbent party spending. However, parties do know how they and their challengers fared at the previous election, and this may shape their calculus. So a challenger in a marginal seat may feel that a strong local campaign will pay dividends, while a challenger in a seat where there is little chance of winning may either be less likely to use all their available resources or less likely to seek the resources necessary for a very active campaign. Similarly, mutatis mutandis, an incumbent party might put greater effort into its local campaigns in seats where its hold on power at the previous election was marginal than in seats where it won by a wide margin. Furthermore, the possibility remains that even within the constraints of the (roughly) four-week campaign period and the limits on constituency campaign spending, parties will react dynamically to the campaign efforts of their rivals in the manner Jacobson describes for U.S. elections.

To get around the problem of simultaneity in the equations, we have reestimated the equations in Tables 3 and 4, holding constant the influence of the preexisting state of party competition in each constituency on the amount spent there. We have achieved this by first regressing party campaign spending in each constituency at a particular election against the state of the parties there after the previous election. (Between the 1979 and 1983 elections, parliamentary constituency boundaries were substantially revised. For the 1979 election results, therefore, we use estimates of the probable results of the 1979 election as if it had been fought in the 1983 seats [BBC/ITN 1983].)

$$\textsc{spendpc}_t = c + b_1V_{t-1} + b_2M_{t-1} + b_3\textsc{second}_{t-1} + b_4\textsc{spendr1pc}_t + b_5\textsc{spendr2pc}_t$$

where

$\textsc{spendpc}_t$ is the percentage of the legal maximum spent by a party at election t;

V_{t-1} is the vote for the party in the seat at the previous election, $t - 1$, as a percentage of the constituency electorate;

M_{t-1} is the percentage point difference between the electorate shares of the party and the winner in the seat (or, where the party won, between it and the second-placed party) at the previous election, $t - 1$—that is, it is a measure of the marginality of the seat: marginality is defined always to be positive, so that the closer the marginality score is to 0, the closer the competition for the party in that seat, and the further it is from 0, the safer the party is in seats that it holds and the further out of contention it is in seats where it is a challenger;

$\textsc{second}_{t-1}$ is a dummy for the party in second place at the previous election: in seats defended by the Conservatives, it takes the value 1 where Labour was in second place, and 0 for other parties; in seats defended by Labour, it is coded 1 where the Conservatives were second, 0 otherwise;

$\textsc{spendr1pc}_t$ is the percentage of the legal limit spent by one rival party at election t;

$\textsc{spendr2pc}_t$ is the percentage of the legal limit spent by the other rival party at election t; and

b_1 to b_5 are slope coefficients.

These equations yield predicted values for local campaign spending (PSP), which are then substituted into the equations of Tables 3 and 4 in place of the actual spending levels. Because the predicted spending values are uncorrelated with the error term, they will give us a clearer indication of the impact of spending at a *particular* election. Therefore, if local campaigning does have an effect on the vote, *once we take into account the extent to which campaign spending is a reaction to the strategic position facing each party before polling day*, we would expect that increased spending by a party should be associated with increasing shares of the electorate for that party and decreasing shares for its rivals. The following equation was estimated for the second-stage models.

$$\textsc{votepc} = c + b_1\text{PCSP} + b_2\text{PLSP} + b_3\text{PASP} + b_4\textsc{semi/unskilled} + b_5\textsc{mine} + b_6\text{Council} + b_7\text{NCWP} + b_i\textsc{region}_i$$

where

PCSP is the predicted percentage of the legal maximum spent by the Conservative party, after controlling for the strategic position in each seat after the previous election;

PLSP is the predicted percentage of the legal maximum spent by the Labour party, after controlling for the strategic position in each seat after the previous election;

PASP is the predicted percentage of the legal maximum spent by the Labour party, after controlling for the strategic position in each seat after the previous election;

$\textsc{semi/unskilled}$ is the percentage of the population in each constituency in semi- and unskilled jobs (the core working class) at the 1981 census;

$\textsc{mine}$ is the percentage of a constituency's population employed in the mining and energy industries in 1981;

$\textsc{council}$ is the percentage of the local population in each seat living in accommodation rented from the local council (a form of state housing);

$\textsc{ncwp}$ is the percentage of the local population in 1981 living in households headed by someone born in the New Commonwealth or Pakistan; and

$\textsc{region}_i$ represents the dummy variables for the regions used in the earlier analyses: once again, the Rest of the West Midlands region serves as a comparator and is contained in the constant term.

The main independent variables have all been linked, in previous studies, with high local levels of support for the Labour party and low levels of support for the Conservatives. Once again, separate analyses were carried out for seats defended by the Conservative party at each election and for seats defended by Labour. In this way, we are able to get a more accurate picture of the effect of incumbency upon campaign effectiveness and to investigate whether there were differences in the "incumbent effect" between the two major parties.

Stage 1: The Determinants of Party Spending

Two main features emerge from the first-stage equations (Tables 5 and 6). Firstly, the equations were much less successful in predicting party spending in the seats a party was defending than in the seats where the party was challenging. For instance, in seats defended by the Conservatives, R-squared for Conservative spending was consistently at .11 or below. In the case of spending by Labour in Conservative seats, R-squared was appreciably larger, at between .62 and .69. For the Liberals, the range was greater, from .24 to .54. A similar pattern held in seats defended by Labour: R-squared for Labour was always under .19, while R-squared for spending by the Conservatives was between .40 and .53, and that for the Liberals was between .28 and .42. Challenger spending is more predictable (in terms of the previous state of the parties in the seat) than is incumbent spending. But clearly a large random element remains in the spending patterns. The second feature to emerge from the first-stage equations concerns the equation coefficients themselves. Seat marginality was the variable most consistently linked to spending.

We would expect that the better its prospects in a seat (i.e., the *smaller* the gap between the challenger and the incumbent), the more a challenger party should spend there. This is borne out by the negative relationships between the marginality of the seat for the challenger and challenger spending: the more marginal a seat was for the challenging party, the more it spent there. There is some evidence to suggest that Conservative incumbents also reacted to the closeness of the contest in a seat by increasing their spending in the most marginal constituencies: the closer the main challenger party was to them at the previous election, the more Conservative incumbents spent at the current contest (Table 5). This was not the case for Labour incumbents: the relationship between marginality and Labour spending in Labour seats was not significant (Table 6). So far, these results seem to suggest that arguments for the effectiveness of campaign spending in U.S. congressional elections also hold for British general elections.

In seats defended by the Conservatives, spending by the two main challengers was affected not only by the marginality of the seat but also by how well the challenging party had done at the previous election (Table 5). The larger the share of the vote won by either Labour or Liberals at the previous election, the more that party spent on its local campaign in the current contest in Conservative-held seats. In addition, Labour generally spent more, and the Liberals spent less, in Conservative-held seats where Labour was the second-placed party, other things being equal.

In both Conservative- and Labour-held seats, there is support for the claim that parties tailor their local campaign efforts according to not only their previous electoral fortunes in the seat but also to how strong a challenge they faced from their rivals. In general, other things being equal, parties spent more on the campaign in seats where their rivals were also spending heavily. This was particularly clear in Labour-held seats, where all three parties' campaign spending was linked in each election to their rivals' campaigning. It was, however, less clear in Conser-

TABLE 5

The Impact of Party Strength, Marginality, Second-placed Party, and Rivals' Campaign Spending on Party Campaign Spending in Seats Held by the Conservatives

INDEPENDENT VARIABLES	PERCENTAGE SHARE OF THE ELECTORATE CONSERVATIVE	 LABOUR	 LIBERAL
Election year 1983 (n = 360)			
Party strength 1979	−.033	1.502	1.064
	(.238)	(.323)	(.403)
Marginality 1979	−.219	−1.165	−.698
	(.141)	(.209)	(.287)
Lab second 1979	−2.618	−10.441	−3.382
	(1.466)	(2.808)	(3.399)
Con spending 1983	—	.170	.308
		(.076)	(.102)
Lab spending 1983	.048	—	.150
	(.034)		(.057)
Lib spending 1983	.092	.083	—
	(.026)	(.038)	
Constant	84.206	37.024	36.346
R^2	.11	.62	.24
Election year 1987 (n = 397)			
Party strength 1983	.459	.767	.949
	(.195)	(.371)	(.373)
Marginality 1983	−.577	−1.182	−1.440
	(.125)	(.214)	(.285)
Lab second 1983	−1.727	7.038	−15.376
	(1.517)	(2.688)	(3.343)
Con spending 1987	—	.120	.122
		(.076)	(.107)
Lab spending 1987	.035	—	.065
	(.029)		(.064)
Lib spending 1987	.029	.128	—
	(.022)	(.034)	
Constant	76.775	58.807	65.763
R^2	.08	0.69	0.32
Election year 1992 (n = 376)			
Party strength 1987	.149	1.151	1.731
	(.169)	(.438)	(.386)
Marginality, 1987	−.304	−.899	−.818
	(.097)	(0.251)	(.272)
Lab second 1987	−1.499	8.007	−17.260
	(1.388)	(3.280)	(3.483)
Con spending 1992	—	.054	.336
		(.107)	(.127)
Lab spending 1992	.023	—	−.002
	(.169)		(.057)
Lib spending 1992	.029	.098	—
	(.019)	(.041)	
Constant	87.493	53.465	12.584
R^2	.07	.65	.54

Note: Data are unstandardized regression coefficients (OLS). Standard errors are in parentheses.

vative-held seats, where the incumbent party tends to spend close to the legal maximum in any case.

Stage 2: Correcting for Simultaneity

Once the influence of the strategic position facing each party in each seat at the outset of the election campaign is taken into account, we are in a better position to assess the links between campaign spending and votes for incumbents and challengers. Tables 7 and 8 repeat the analyses reported in Tables 3 and 4, replacing the actual spending levels with the predicted spending levels from our analyses.

The results broadly bear out our earlier conclusions. In most cases, spending by the nonincumbent parties in a seat does have a significant effect on their election results, and in the expected direction. Even when we take into account the impact of likely calculations based on parties' chances in a seat, the

TABLE 6

The Impact of Party Strength, Marginality, Second-placed Party, and Rivals' Campaign Spending on Party Campaign Spending in Seats held by Labour

INDEPENDENT VARIABLES	SPENDING AS PERCENTAGE OF LEGAL MAXIMUM: CONSERVATIVE	LABOUR	LIBERAL
Election year: 1983 (n = 260)			
Party strength 1979	.697	−.349	−.529
	(.365)	(.356)	(.638)
Marginality 1979	−1.060	.111	−1.479
	(.240)	(.220)	(.350)
Con second 1979	.670	−11.976	−1.302
	(5.834)	(5.421)	(8.178)
Con spending 1983	—	.172	.213
		(.060)	(.079)
Lab spending 1983	.177	—	.242
	(.063)		(.094)
Lib spending 1983	.133	.091	—
	(.042)	(.042)	
Constant	36.459	89.804	67.597
R^2	.53	.19	.28
Election year: 1987 (n = 209)			
Party strength 1983	.806	.001	−1.106
	(.566)	(.471)	(.699)
Marginality 1983	−.904	−.133	−1.811
	(.358)	(.299)	(.366)
Con second 1983	−.520	−.448	−7.288
	(4.126)	(3.023)	(3.898)
Con spending 1987	—	.118	.234
		(.058)	(.066)
Lab spending 1987	.163	—	.216
	(.084)		(.086)
Lib spending 1987	.240	.131	—
	(.066)	(.056)	
Constant	30.987	71.054	63.445
R^2	.40	.10	.34
Election year: 1992 (n = 229)			
Party strength 1987	.896	−.203	1.275
	(.479)	(.389)	(.453)
Marginality 1987	−.446	−.107	−.684
	(.276)	(.219)	(.217)
Con second 1987	5.218	−4.408	−9.092
	(4.066)	(2.827)	(3.338)
Con spending 1992	—	.200	.161
		(.051)	(.055)
Lab spending 1992	.341	—	.065
	(.083)		(.076)
Lib spending 1992	.229	.052	—
	(.070)	(.055)	
Constant	12.155	81.006	17.235
R^2	.44	.18	.42

Note: Data are unstandardized regression coefficients (OLS). Standard errors are in parentheses.

TABLE 7

The Impact of Constituency Campaign Spending on Party Share of the Electorate: Predicted Spending from Table 5: Seats Defended by the Conservatives

INDEPENDENT VARIABLES	PERCENTAGE SHARE OF THE ELECTORATE: CONSERVATIVE	LABOUR	LIBERAL
Election year 1983 (n = 360)			
Predicted Conservative spending	−.244	−.342	.246
	(.073)	(.065)	(.090)
Predicted Labour spending	−.094	.278	−.081
	(.132)	(.012)	(.016)
Predicted Liberal spending	.002	−.042	.202
	(.019)	(.017)	(.024)
R^2	.75	.88	.60
Election year 1987 (n = 397)			
Predicted Conservative spending	.274	−.389	.407
	(.066)	(.073)	(.094)
Predicted Labour spending	−.190	.343	−.093
	(.010)	(.011)	(.015)
Predicted Liberal spending	−.139	−.036	.193
	(.014)	(.015)	(.019)
R^2	.85	.93	.75
Election year 1992 (n = 376)			
Predicted Conservative spending	−.196	−1.282	1.520
	(.119)	(.153)	(.178)
Predicted Labour spending	−.244	.486	−.207
	(.017)	(.022)	(.026)
Predicted Liberal spending	−.101	.053	.066
	(.160)	(.021)	(.024)
R^2	.82	.91	.80

Note: Data are unstandardized regression coefficients (OLS) controlling for % semi/unskilled manual workers, % workers in mining and energy, % council tenants, % with household head born in New Commonwealth, and region. Standard errors are in parentheses. The table does not report the full regression equations but is limited to the coefficients for spending: full details of the equations, including all control variables, are available on request.

TABLE 8

The Impact of Constituency Campaign Spending on Party Share of the Electorate: Predicted Spending from Table 6: Seats Defended by Labour

INDEPENDENT VARIABLES	PERCENTAGE SHARE OF THE ELECTORATE		
	CONSERVATIVE	LABOUR	LIBERAL
Election year 1983 (n = 260)			
Predicted Conservative spending	.281	−.077	−.028
	(.024)	(.030)	(.035)
Predicted Labour spending	.006	−.120	.184
	(.051)	(.064)	(.074)
Predicted Liberal spending	−.059	−.080	.028
	(.028)	(.036)	(.042)
R^2	.85	.71	.32
Election year 1987 (n = 209)			
Predicted Conservative spending	.319	−.177	−.017
	(.040)	(.030)	(.093)
Predicted Labour spending	−.334	.148	.268
	(.092)	(.069)	(.093)
Predicted Liberal spending	−.033	−.131	.100
	(.032)	(.024)	(.033)
R^2	.77	.87	.43
Election year 1992 (n = 229)			
Predicted Conservative spending	.232	−.106	.004
	(.027)	(.026)	(.017)
Predicted Labour spending	.249	−.070	−.162
	(.070)	(.066)	(.045)
Predicted Liberal spending	−.170	−.084	.244
	(.028)	(.026)	(.018)
R^2	.77	.76	.66

Note: Data are unstandardized regression coefficients (OLS), controlling for % semi/unskilled manual workers, % workers in mining and energy, % council tenants, % with household head born in New Commonwealth, and region. Standard errors are in parentheses. The table does not report the full regression equations but is limited to the coefficients for spending: full details of the equations, including all control variables, are available on request.

more a challenger spends, the more votes it gets, and the fewer votes its rivals win. But for incumbent parties the picture is much more complex. In some cases, spending by the incumbent party is related to electoral performance in the expected ways. For instance, the more the Conservatives spent in their own seats on their constituency campaign in 1987, the more votes they won. However, in other cases, incumbent spending was not significantly related to electoral outcomes (as with Conservative spending and Conservative votes in Tory-held seats at the 1992 election). And in still other cases, the relationship between incumbent spending and party performance actually ran counter to expectations, such that the more a party spent in its own seats, the worse it did and the better its rivals performed. (For instance, in seats defended by the Conservatives in 1983, the Conservative vote fell as the party spent more on the campaign, other things being equal, while the Liberal vote grew.) This latter situation is consistent with Jacobson's argument that high spending by an incumbent can be a sign of electoral weakness (coming under strong and sustained challenge) rather than a sign of strength. However, as we shall now argue, this apparent similarity may be a result of factors unique to the British situation, stemming from the tight legal controls on local campaign spending.

These analyses still beg a question: Why did incumbent spending have such a mixed impact on support? A clue can be found in the very weak links between incumbent spending and the state of the party competition in each seat (Tables 5 and 6). This seems to suggest either that incumbent spending is highly random or that it does not vary much.

To investigate this further, we have looked at how the parties spent in all seats and in seats defended by each main party. On average, parties spent well above their national average in seats they were defending (Table 9). What is more, however, and consistent with the suggested scenario, it is clear from an examination of the standard deviations that there was markedly less variation in a party's spending in its own seats than there was in the same party's spending in seats where it was a challenger. (That said, an examination of the standard deviations for the various parties reveals that there was less variation in spending among Conservative candidates defending Conservative seats than among Labour candidates defending Labour seats; the main exceptions occur in seats defended by the Liberal–SDP Alliance or the nationalists, but these were only a handful of constituencies.) Not only were parties spending above average in their own seats, therefore, but they tended to spend at similar levels in *all* the seats they were defending. The lack of an incumbent campaign effect, therefore, seems to be an artifact of relatively uniform campaign effort by all incumbent MPs, rather than of a distinct "incumbent advantage" offset by strong challenges. (However, there is one caveat. Because there is little variation in spending by incumbent parties, we are not able to say clearly what would happen if an incumbent party spent little or nothing on its campaign: this would take us beyond the range of the data.)

Where a party was not defending a seat, however, there was much more variation in how much it spent. What is more, as we have already seen, challenger spending was significantly related to the marginality of each seat: the better a challenging party's chances in a seat, the more it spent, and the worse its chances, the lower its spending. The implication, therefore, seems to be that parties husband their resources carefully and spend "wisely." This may well be linked to the health of local parties: where a party is far from any realistic chance of ever winning a seat, it is liable to be small and weak and to be unable and/or unwilling to raise and spend the finances necessary to fight a full local campaign. Where a party is defending a seat, however, it has all the advantages of office and is probably relatively strong in terms of

TABLE 9

Party Spending as Percentage of Legal Maximum Controlling for Incumbency

	ELECTION YEAR					
	1983		1987		1992	
SPENDING	MEAN	S.D.	MEAN	S.D.	MEAN	S.D.
Conservative						
Britain	74.06	22.41	78.26	22.74	79.05	22.66
Con seats	85.93	11.10	89.19	9.67	91.00	7.88
Lab seats	57.07	23.56	56.84	25.54	58.28	25.07
Lib seats	90.37	6.90	85.86	20.29	86.91	11.51
Nat seats	72.55	15.38	69.42	5.75	91.80	9.31
F-score	142.07		167.17		188.38	
Labour						
Britain	66.53	23.78	70.87	24.31	70.58	24.80
Con seats	58.75	24.60	65.47	25.72	65.38	26.81
Lab seats	76.81	17.55	80.92	17.25	80.50	16.25
Lib seats	67.44	30.51	71.16	26.22	62.09	28.77
Nat seats	96.08	3.04	80.28	22.22	48.97	30.32
F-score	36.34		20.34		23.01	
Alliance (Liberal Democrat)						
Britain	56.23	26.86	60.49	28.83	42.72	30.67
Con seats	63.69	23.23	69.88	24.33	53.05	28.10
Lab seats	45.18	27.58	39.11	24.93	22.22	21.71
Lib seats	89.02	8.81	94.58	4.48	89.69	16.79
Nat seats	29.37	9.90	50.50	33.41	21.14	8.42
F-score	34.42		89.91		92.68	

Note: Liberal is used here as shorthand for the center party. In 1983 and 1987, this was the Liberal-Social Democratic Party Alliance; in 1992, it was the Liberal Democrats. The figures for *nationalist* campaign spending refer to both Plaid Cymru (the Welsh nationalist party) and the Scottish National Party. The figures refer to Scottish and Welsh seats only. Sig F = 0.00; n = 633.

its local organization and fundraising capacities: it therefore seems to spend as much as it can on the local campaign, whether or not it faces a serious challenge.

CONCLUSION

Our analyses of spending in the constituencies at the last three general elections in Great Britain show that the country's political parties target their money-raising and campaign-spending activities on winning marginal seats and defending those which they already hold. These strategies are positively linked to their vote winning: in general, the more that a party spends, the more votes and seats it gets; and although this is countered somewhat by the negative impacts of its opponents' spending, overall a party gains more from its own expenditure on the campaign (most of which goes on campaign literature) than it loses from expenditure by others. However, the main beneficiaries from campaign spending are parties challenging in a seat, not the incumbent parties themselves. The latter seem to gain only mixed advantages from their campaign spending.

The causal relationship behind these findings cannot be determined from the available data. Two possible mechanisms are involved:

1. Money spent on campaigning, most of which goes on the preparation and dissemination of information to the electorate, "buys" votes in the same way that advertising commercial products increases a company's "market share."
2. The money spent by a party on a constituency campaign reflects the strength and commitment of the local party organization there and is thus an indicator of its overall campaign effort.

In the first case, money is crucial because its expenditure is directly related to vote winning. In the latter it is not, because spending is merely a surrogate for campaign effort generally; and it may be other activities (e.g., canvassing on the doorstep) that improve turnout and win converts. We cannot evaluate the two, but it is very unlikely that the second is not at least a partly accurate representation (Pattie et al. 1994).

For analysts of British elections, the findings reported here suggest that an important rethink concerning the constituency campaign is in order. The conventional wisdom as advanced by the influential Nuffield election studies has been that the local campaign has no impact upon the outcome of elections and serves merely as a ritual activity for local party activists. Clearly, this is no longer a tenable case: the local campaign, while not as widely publicized or as glamorous as its national counterpart, still plays an important function in shaping the outcomes of British general elections.

Notes

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1. The only substantial change in constituency boundaries between 1983 and 1992 involved the division of the Milton Keynes seat into two constituencies prior to the 1992 election. The new seats are therefore excluded from analyses of the 1992 election. In some later analyses, we employ constituency data from the 1979 election. While that election was fought using a set of local constituency boundaries different from those of the election of the 1980s, we have access to estimates of the 1979 results for the seats used in 1983 (BBC/ITN, 1983). It is these estimates that we use in the analysis.

2. The corrupt practices of prereform electioneering provided ample comic material for several nineteenth-century British novelists. Notable examples include John Galt's *Provost*, chap. 5; Charles Dickens' *Pickwick Papers*, chap. 13; and Anthony Trollope's novels, notably *Ralph the Heir*, chap. 29, based on his own experiences as a candidate at Beverley.

3. During the period covered by this study, center party politics changed. At the 1979 election, the Liberals were the main center party. By 1983, they had been joined by a breakaway group from the Labour party, forming the Liberal–SDP Alliance. After the 1987 election, the Alliance broke up acrimoniously, leaving two "rump" parties, the Liberals and the SDP, neither of which survived long as electoral forces. The remainder of the old Alliance went on to found a new center party, the Liberal Democrats. For consistency, we refer to the main center party (the 1979 Liberals, the 1983–87 Alliance, and the 1992 Liberal Democrats) as the Liberals in all tables. Throughout the text, we refer to the main center party organization operating at the time of each election.

4. The regions employed in the analysis were Strathclyde, East Central Scotland, Rural Scotland, Rural North of England, Industrial North East England, Merseyside, Greater Manchester, Rest of the North West England, West Yorkshire, South Yorkshire, Rural Wales, Industrial South Wales, West Midlands Conurbation, Rest of West Midlands, East Midlands, East Anglia, Devon and Cornwall, South West (excluding Devon and Cornwall), Inner London, Outer London, Outer Metropolitan, and Outer South East.

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BEYOND TOCQUEVILLE, PLEASE!

Rogers Smith in "Beyond Tocqueville, Myrdal and Hartz: The Multiple Traditions in America" in the September 1993 issue of this Review *argues that ascriptive inequalities (eg. racism, sexism, nativism) are neither mere deviations from liberalism nor only symptoms of liberalism. Rather, multiple ideologies coexist in uneasy tension. Jacqueline Stevens criticizes Smith for failing to attend to the work of W. E. B. Du Bois and others—whose descriptions of American ideological history, she says, provide the same insight that Smith claims as his own. She goes on to discuss how defining a "mainstream" of scholarship shapes inferences as to what counts as knowledge and further suggests that liberalism and exclusion betray an underlying consistency. In his reply, Smith recognizes the contributions of Du Bois and others, but argues that although they do foreshadow his work, they do not offer the same critique or do the same job. And he asserts that the linkages between liberalism and ascriptive inequality are political, economic, social and psychological—not logical—ones.*

COMMENT

Rogers Smith's "Beyond Tocqueville, Myrdal, and Hartz: The Multiple Traditions in America" (1993) both speaks to and embodies several deficiencies in the explication of American political thought. The most troublesome problems are the implicit and sometimes explicit claims to originality. Similarly, by virtue of publishing this as a lead article, the *American Political Science Review* signals that its authors do not have to be responsible for a full spectrum of relevant political writings, but only those that dominate Hartzian-inspired reading lists. Smith's silence on others who have preceded him suggests epistemological missteps, not simple bibliographical ones. I want to be clear that while Smith is the immediate focus of my attention, I see the article as symptomatic of far broader difficulties in the construction of knowledge and history. For tacitly urging what I shall argue is a politically constituted "mainstream" into different avenues of inquiry, however, Smith is to be commended.

As I read the essay, Smith is claiming that all scholarship in American political thought makes one of two mistakes. First, racism and sexism are regarded as deviant sidesteps in the otherwise forward march of liberal ideas in America's political culture. This is the fault of Alexis de Tocqueville (1969), Gunnar Myrdal (1944), Louis Hartz (1955), and anyone who takes their claims seriously. Smith's article begins, "Since the nation's inception, analysts have described American political culture as the preeminent example of modern liberal democracy, of government by popular consent with respect for the equal rights of all" (1993, 549). Second, in contradiction to his first claim, Smith notes books in which racism and sexism *are* treated as central aspects of American political culture, but these works wrongly suggest that inegalitarian ideologies are of a piece with the liberal nation-state: the "recurring admixture" of racist, sexist, and liberal values "does not prove that ascriptive inegalitarian outlooks have been *logically* compatible with liberal democracies" (p. 556, emphasis original).[1] Although some writers do recognize that racism is a crucial feature of American politics (e.g., writers in Critical Race Studies) and recognize that this is not at one with liberal aspirations, these texts are "rare and do not yet extend to explicit critiques of Tocquevillian frameworks or to any developed alternatives" (ibid.). Smith offers to prove for the first and definitive time that racism and sexism are a part of this country's political culture and so is liberalism and that the ideologies are incompatible. Smith labels this theoretical insight the *multiple traditions thesis*: "At its heart, the multiple traditions thesis holds that the definitive feature of American political culture has been not its liberal, republican, or 'ascriptive Americanist' elements but, rather, this more complex pattern of apparently inconsistent combinations of the traditions, accompanied by recurring conflicts" (p. 558).

Smith's expository discussions as well as his thesis misinform. First, sweeping claims about the hegemony of Tocqueville, Hartz, and Myrdal are naive and wrong, as Smith himself illustrates in his use of both historical and political theory texts that specify how the narratives of his troika are incorrect. So why is Smith saying that analysts tell a Pollyanna-ish version of our country's history when his own references, published under the imprimatur of university presses, falsify this claim? At the heart of the error is the status of what Smith calls *mainstream approaches* (or "conventional narratives" or "recent major works" [1993, 550, 556]), which are defined only implicitly, and circularly at that: Tocquevillian approaches are mainstream and mainstream approaches are Tocquevillian. This is the unconquered terrain that justifies his expedition. Smith never explains the formation of this mainstream scholarship or its relation to credible but nonmainstream thought. So Smith is never prompted to consider why his intervention will be a mainstream one, while other scholarship making similar points is not. Yet this distinction is really at the crux of his article, which offers no significant original arguments, but offers old ones to a mainstream (i.e., white, male-dominated) forum. Smith's essay is not published in the *APSR* because it is innovative to the mainstream (lots of people have made Smith's critique, and far more persuasively, including some of those Smith cites). Rather, his ideas are innovative to the mainstream because they appear in the *APSR*.

"Analysts" do not write a monolithic narrative of American political culture, as Smith's introductory paragraph implies (1993, 549). Rather, some people write some things, and some people write other things. For some reason, Smith and mainstream scholarship choose not to discuss the views of American political culture held by W. E. B. ("The-problem-of-the-twentieth-century-is-the-problem-of-the-color-line") Du Bois. Smith refers to Du Bois just once, to say that he was a Lamarckian (p. 562). Smith's silence on Du Bois, who engages in precisely the kinds of analyses Smith believes are absent, is disturbing. What would we make of an article on the scholarship of American political culture that mentioned Tocqueville only to say that he believed aristocrats innately superior? In any case, although at times Du Bois discusses the relation between the "soul" of a people and its bloodlines, the source of this is more Hegelian than Lamarckian.[2] And Du Bois's insistence on the social determinations and manifestations of race becomes more pointed over time. In *The Negro* ([1915] 1975b) he writes that race is not a scientific category (1975b, 13). In *Dusk of Dawn* ([1940] 1992), after Du Bois destroys all conventional definitions of race, the composite interrogator, Van Dieman, asks, "But what is this group; and how do you differentiate it; and how can you call it 'black' when you admit that it is not black?" Du Bois responds, "I recognize it quite easily and with full legal sanction; the black man is a person who must ride 'Jim Crow' in Georgia" (1992, 153). Unless the giraffe is the animal that under legal sanction must eat from tall branches, Smith's allusions to Lamarckianism mislead.

Throughout his career Du Bois was especially attentive to the fluctuations and paradoxes of American political culture, albeit many of his earlier writings (though not all) suggest a more optimistic *telos* than that which Smith holds. However, in *Dusk of Dawn* (1992 [1940]), one of his more widely read books, Du Bois restates his 1905 announcement of the formation of the Niagara movement, which describes the "new American creed: fear to let black men even try to rise lest they become the equals of the white. And this is the land that professes to follow Jesus Christ" (1992, 90). At the point at which Du Bois is setting down this memoir, he has concluded that the White World is shot through with antonymies, which pose a "dilemma" for the White Man (153–54, 160). After laying out the contradictory "codes" of the Christian–Gentleman–American–White Man, Du Bois writes:

> The trouble was, however, that when my friend tabulated all of the codes which he at once and apparently simultaneously was to put in action, he found a most astonishing reality and here it is:
>
> | *Christian* | *Gentleman* | *American* | *White Man* |
> |---|---|---|---|
> | Peace | Justice | Defense | War |
> | Good Will | Manners | Caste | Hate |
> | Golden Rule | Exclusiveness | Propaganda | Suspicion |
> | Liberty | Police | Patriotism | Exploitation |
> | Poverty | Wealth | Power | Empire |
>
> Looking them over he doesn't know what on earth to do. It is not only a dilemma, it is almost a quadri-lemma. (pp. 164–65)

Du Bois here recognizes the tenacious survival of analytically separate ideologies in the United States. Clearly and self-consciously, Du Bois neither privileges one column as "truly" American (the move for which Smith faults Tocquevillians) nor adduces any ultimate coherence of this ideological system. Instead, his interlocutor says: "It's all both reasonable and impossible. Take each column alone and it is to me absolutely convincing. I believe in it" (p. 165). The Christian–Gentleman–American–White Man goes on simply to restate the content of each column, conceding that their dynamics are "not logical, correct, compelling" (p. 167). The "quadrilemma" just *is*. It is not surprising that the terms of this dilemma were popularized by Gunnar Myrdal (1944), whose *American Dilemma* includes more references to Du Bois than to any other single individual.[3]

Earlier in this chapter, Du Bois, again prefiguring Smith, describes the ascriptive norms producing the incompatible codes, in which racism seems to have gotten the upper hand over liberalism. This is expressed, he writes, in the "deep convictions of myriads of men that *congenital differences* among the main masses of human beings absolutely conditioned the individual destiny of every member of a group" (1992, p. 139, emphasis mine; see also p. 137). Du Bois distinguishes the values of liberalism and democracy from those of ascriptive norms of exclusivity and exploitation. The columns do not map exactly onto Smith's explication (e.g., Du Bois equates "American" with strictly racist and militarist ambitions), but the effort to locate ascriptive inequality and inclusivity in the same "contradictory" tradition provides an obvious (but perhaps nonmainstream?) alternative to the Tocquevillian narrative—one that clearly foreshadows the spirit of Smith's project.[4] Instead of writing that "leading accounts of American political culture are inadequate" because they overlook Smith's insight that "America has never been completely liberal" (1993, 550), Smith might attend to Du Bois and then explain how it is that this interpretation of political history—and those of other prominent American radical writers such as Emma Goldman (1970), Randolph Bourne (1977), C. Wright Mills (1956), Howard Zinn (1980), and Angela Davis (1981)—have gone unrecognized and uninstitutionalized by people such as himself. But these accounts of political culture and history have not gone unwritten, as the first sections of Smith's article would suggest.

My intention is not to issue a pious plea for researchers of race and gender politics to incorporate and evaluate academic contributions from members of the groups they study (as the list of the nonmainstream authors above makes clear). If Smith were to undertake a critique of ascriptive inequalities, there would be no reason to begin with the work of Angela Davis, rather than the practices and institutions he would like to study. However, Smith does not pursue such an inquiry but presumes to criticize the country's *intellectual* tradition. And here the systematic exclusion of a good portion of that tradition is academically indefensible and only reinforces racist, sexist, and Hartzian ideas about which texts are worthy of intellectual scrutiny.

Smith's survey of the ideological terrain at the country's founding suffers a similar limitation. He overstates the case when he writes that the "comparative moral, material, and political egalitarianism that prevailed at the founding among moderately propertied white men was surrounded by an array of other fixed, ascriptive systems of unequal status, *all largely unchallenged by the American revolutionaries*" (1993, 549, emphasis mine). Historian John Hope Franklin describes the efforts to include anti-slave-trade and antislavery language in the Declaration of Independence and writes, "As the colonists saw in England's new colonial policy a threat to the economic and political freedom that they had enjoyed for several generations, they also seemed to realize a marked inconsistency in their position as oppressed colonists *and* slaveholders" (1967, 126–27). Abigail Adams wrote her husband, "It always appeared a most inequitous scheme to me to fight for ourselves what we are daily robbing and plundering from those who have as good a right to freedom as we" (quoted in Franklin 1967, 128–29). This is a theme that is also pursued by Winthrop Jordan (1968, 386). Smith does not explain why he finds Franklin and Jordan's evidence unconvincing. Obviously whatever challenges were made at this point were unsuccessful, but this does not mean that they were not offered. The descriptions of these arguments by Franklin and Jordan—mainstream historians—do not allow even for the weaker claim, that inegalitarian positions were "largely" unchallenged.

Smith's second mistake is a methodological one: he encourages political theorists to parrot the superficial inconsistencies of what they study. Criticizing Michael Rogin's book on Jacksonian democracy (which Smith inexplicably characterizes as "postmodernist"), he maintains that racism and sexism are strictly antithetical to liberal egalitarian ideals.[5] He never actually argues this point but uses the observation that "people operate for long periods while holding contradictory beliefs" as evidence for his claim that this is what is happening in the case of American history. Had Smith reviewed the historical and theoretical literature on the relation among democratic practices and those of inequality and exclusion, beginning with Thucydides' *Peloponnesian War* and other studies of the colonialist, slave, and democratic institutions of ancient Greece, he would have discovered that his own intuitions are not dispositive. Carl Schmitt makes the case that democracies especially require norms of exclusions, in order to make the equality that they offer meaningful: "The more equal a country is, the more it seeks to exclude those who do not belong to the state" ([1923] 1984, 12). Hannah Arendt writes of the *paradox* (not *contradiction*) of the Rights of Man emerging at the same moment as the particularist nation-state (1958, 291). Numerous historians have argued that the national unity of the United States was *furthered* (not undermined, as Smith suggests) by its racial exclusions. For instance, Rubin Weston writes that the "reconciliation of the North and South" occurred at the "expense of a dissimilar race" as both united in opposition to disenfranchised blacks and colonized populations in the Philippines and elsewhere (Weston 1972, 32). And Alan Dawley writes: "It was no accident that state-sanctioned racial exclusions [e.g., the Exclusion Act of 1882] went hand-in-hand with formal equality before the law. Although these exclusionary practices obviously violated the liberal creed of open competition on one level, in a deeper sense exclusion by race was its logical concomitant" (1991, 30).[6] Dawley argues that the exclusions of race legitimated domestic inequality and that this internal racism served to solidify the country as a nation-state both internally and for purposes of imperialism. Dawley does what any good scholar does: he lifts the shroud of what even Smith terms "*apparently* inconsistent" dynamics to explore a deeper logic (Smith 1993, 558, emphasis mine). Now it very well may be the case that Rogin, Thucydides, Schmitt, Arendt, Rubin, and Dawley are wrong (although I don't happen to think so), but they are not so obviously wrong that Smith can simply ignore these arguments and expect us to believe that inegalitarian and exclusionary practices are inconsistent with democratic values just because Smith says so.

Finally, Smith's account of the status of Critical Race Studies is an odd one. The single paragraph on this literature refers to one article by one scholar and then quotes one phrase to serve as a proxy for a diverse, complicated field. Why say that "these arguments are rare" (Smith 1993, 556) when numerous prominent legal scholars at prestigious law schools make them to our next generation of legal scholars,[7] when they appear regularly in prestigious law review journals, and when they are circulated in widely read books? Harvard University Press's customer service department says that Patricia Williams's (1991) *Alchemy of Race and Rights*, unmentioned by Smith, has sold over 41,000 copies in its first four years—a blockbuster by academic standards and hardly symptomatic of a field that is weak or marginal.

Smith believes that Critical Race Studies arguments do not accomplish the task that he sets forth because they do not "yet extend to critiques of Tocquevillian frameworks or to any developed alternatives" (1993, 556). Leaving aside the last part of the sentence (which implies that these scholars are writing in a vacuum), we find Smith dissatisfied with accounts of race that do not explicitly critique Tocqueville. Smith never explains why a critique of "Tocquevillian approaches" is more disruptive of a corrupt intellectual tradition than a radical account of American politics undertaken through the lens of, for instance, slavery. If people are making arguments inconsistent with, and without regard for, the ones Smith associates with Tocqueville, Myrdal, and Hartz, then a fair conclusion is that parts of the academy are immune from their inroads. Again, this suggests that his narrowing of what counts as the "mainstream" artificially restricts the scope of academic inquiry. Had Smith fully acknowledged the accounts of American history that do not inform the vast majority of articles in the *American Political Science Review,* but are the bedrock of analyses in journals such as *Black Scholar, Phylon, Feminist Studies, Signs,* and the *Journal of American Studies,* then it would be clear that the problem is not a dearth of scholarship but a dearth of insight within a particular readership.

Smith is telling a very specific audience that they ought to wake up and smell the coffee. This is a political injunction, not an academic one: many people have been smelling this coffee for a long time. Smith's silence on the distinction between what is "mainstream" and what is said, his dismissive treatment of the complicated relation between liberal democratic traditions and liberal exclusionary and inegalitarian ones, and his disregard of studies by those who have pursued these questions reiterate the narrow vision of American history held by those Smith presumes to educate. The real conundrum is not that American history contains two heretofore unrecognized and conflicting traditions but that American scholarship pursues two approaches to this history: one explores the tensions of racism, sexism, and egalitarianism in American politics; another follows the mainstream.

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RESPONSE

Jacqueline Steven's critique of my 1993 article is driven by an appropriate concern. She believes that because my article does not refer readers to various related critiques of American political culture and intellectual history, it will reinforce beliefs that those critiques—by authors like W. E. B. Du Bois, Emma Goldman, C. Wright Mills, Angela Davis, and others—need not be read. Thus she thinks my essay exemplifies and compounds the failings of the "mainstream" scholarship I purport to correct. I confess I do not find her argument regarding this "mainstream" wholly clear. At times she seems to say that I am guilty of defining the "mainstream" too narrowly; at others, she seems to be arguing that the "mainstream" is in reality too narrow. But I take her point to be that too many people well positioned in our discipline define the "mainstream" in ways that are both too narrow, in that they include the views only of people like themselves, *and* too influential.[8] This claim seems right to me. I also concede that nonetheless, I did not, in my essay, explicitly urge readers to turn to the sorts of critical writers Stevens invokes. If I thereby authorized the inference that they could be ignored, this warrants the correction Stevens has provided.

Beyond this point, however, Steven's critique is unpersuasive. Given the severe constraints of journal-length articles, I chose to devote my essay first to criticizing my main targets—"conventional" Tocquevillian, Myrdalian, and Hartzian accounts of American political culture—and then to laying out my own *multiple traditions* view. As I indicated then and elaborate here, these sorts of accounts merit extensive critique, because they are still cited as authoritative by a wide range of scholars and their premises are often echoed even in works that appear to present quite different views.

Focusing on these tasks meant that my essay could only summarily contrast its multiple traditions account with those of the many scholars who have also analyzed racism, sexism, ethnocentrism, and other forms of ascriptive inequality in America. Stevens thinks my emphases unjustified on the grounds that there was no "originality" in the case I made, so that I should have paid more attention to those who had anticipated my views, if I chose to write at all.

I thought and still think that I was offering a distinctive position that merited development in its own right. I am grateful for the chance to say a little more now about how my view differs from the scholarship Stevens rightly commends. But in doing so, I do not mean to deny that these authors merit reading—as much or more than anything I write.

I wish I had made that point more clear; and Stevens' inaccurate summary of my article persuades me that I failed to make other key aspects of its argument clear, at least to her. She reads me as saying (1) that "all scholarship in American political thought" treats "racism and sexism as deviant sidesteps"; (2) that I deny that "inegalitarian ideologies are of a piece with the liberal nation-state"; (3) that writers who recognize racism as a crucial feature of American politics are "rare" and that their work has not "extended to critiques of Tocquevillian frameworks or to any developed alternatives"; and (4) that the truth is that racism, sexism, and liberalism are all part of American culture and that "the ideologies are incompatible." Points 1 and 2 misrepresent what I said; points 3 and 4 are correct but must be understood in light of further aspects of my argument that Stevens fails to acknowledge.

Concerning point 1, I shall forevermore be wary of shaving qualifiers to save space. I do think that much Tocquevillian scholarship defines "racism and sexism as deviant sidesteps," a practice to which I object. But Stevens assumes when I wrote that "analysts" had described American political culture in these terms, I meant "all analysts." She then triumphantly accuses me of contradiction when I recognize that some have not. I did not, however, say "all analysts"; and I did not expect to be so construed, precisely because I then specified the analysts I was criticizing and made it clear there were "many others" whose work had "greatly enriched understanding" of racial, ethnic, and gender issues in ways to which my argument was indebted (1993, 555). I nonetheless did not feel anyone had put forth the full case I was making.

Point 2 presents much more important misunderstandings that involve the basic elements of my argument which Stevens overlooks. I do hold that America has been centrally constituted by inegalitarian ideologies that are not simply forms of liberalism (as Hartzians contend) or recognized to be irrational prejudices (as Myrdal argued). And I do offer a thesis about the relationship of America's liberal and democratic traditions to traditions of ascriptive inequality, including racism, sexism, and nativism among others.[9] However, that inegalitarian ideologies are not "of a piece with" the liberal nation-state is not that thesis.

Rather, my thesis is that even though many principles of these traditions are inconsistent with one another as a matter of formal logic, Americans have always blended them together, because they have distinctive intellectual, psychological, and political attractions (Smith 1993, 550, 558). This thesis does not deny that there are linkages between liberal democratic policies and practices and ascriptive inequalities and exclusions in America. On the contrary, I not only recognize how liberal market systems generate class inequalities (p. 556), but I also repeatedly insist that it is "normal, not anomalous" for the pursuit of liberal democratic policies to generate political, economic, social, and psychological conditions that foster the periodic resurgence of traditions of ascriptive inequality (pp. 550, 554, 558–59, 562–63).

My view, then, is not that it is wrong to say that liberalism, democratic republicanism, and ascriptive inegalitarian systems have been in *some* sense "of a piece" in America. It is that the linkages should be described as political, economic, sociological, and psychological, rather than as "logical" entailments. It is precisely because I see these traditions as *all too deeply* linked that I object to describing the United States simply as a "liberal democracy." I see its inegalitarian ascriptive traditions as also deeply constitutive of American national identity.

Stevens seems just not to get all this. She refers to "the liberal nation-state," apparently privileging American liberal elements, the very habit I am challenging. She goes on to contend, without citation, that my insistence on the logical incompatibilities between liberal and ascriptive ideologies means that I believe the "national unity of the United States" has been "undermined," not "furthered," by "racial exclusions." But my argument was *precisely* that the "political and psychological appeal" of ascriptive traditions have been of great value in fostering certain kinds of "national unity." I simply insisted that they were illiberal kinds that excluded many on racial and ethnic grounds and allowed others to be only second-class members of the "national union." I did observe that these inequalities have often been the sources of the country's deepest political conflicts. Repeatedly, however, I underscored that those struggles have often resulted in relatively enduring forms of political union, albeit ones constructed along ascriptive inegalitarian, not purely liberal, lines (1993, 550, 554, 557–59, 562–63).

After missing all these contentions, Stevens culminates this part of her critique by citing Alan Dawley's argument that "exclusionary practices obviously violated the liberal creed . . . on one level." She insists (along with Dawley, Carl Schmitt, and others) that even so, liberal democracy has been connected with "inegalitarian and exclusionary practices" as a matter of "a deeper logic."

Good point—but it doesn't contradict mine. My only quarrel with Dawley's formulation is that I think calling the linkages a matter of "deeper logic" is vague and confusing. I think it more precise to spell out how and why—despite the "obvious" logical contradictions Dawley and others perceive—liberal democratic ideas and practices can generate conditions favorable to ascriptive inegalitarian ideas and practices. To term those sorts of linkages *logical* (as Dawley and Michael Rogin do), rather than political, sociological and psychological, is misleadingly to suggest that the linkages are primarily at the level of shared *formal principles*. I believe that their analyses, like mine, better support the view that at the level of principle, the ideologies are contradictory in important ways: their connections are primarily at different levels of lived experience.

Moreover, I see my particular account of the linkages as preferable to many others, because I identify common factors that explain why diverse inegalitarian ascriptive traditions of American identity can be and often are politically linked, even though doctrines like white supremacy, male superiority, and Anglo-Saxon manifest destiny are far from identical.[10] In contrast to liberalism, democracy, and civic republicanism, which advance the threatening view that political identities are the products of alterable human choices, these traditions all offer powerful assurances that people's political statuses are instead ordained as part of a meaningful transcendental divine or natural order. Such doctrines often are more effective than liberal democratic ones in warding off change by justifying the privileges of the powerful and making the suffering of the disadvantaged seem less arbitrary, maybe even less unbearable to the disadvantaged themselves (Smith 1993, 550, 558, 562).

Perhaps because Stevens simply ignores these "linkage" arguments, she also gives short shrift to my further contentions that the distinctive attractions of America's ascriptive inegalitarian traditions lead to their periodic resurgence in American life after periods of liberalizing, democratizing reforms and that we are in such a period right now. Neglecting those points is a serious omission. Since I wrote the article, the appearance of widely noted books explicitly urging the genetically based intellectual inferiority of blacks (Herrnstein and Murray 1994) and the desirability of keeping America a white nation bound by ties of blood (Brimelow 1995), along with mounting antiintegration, antifeminist, and antiimmigrant political tides, have abundantly confirmed those depressing but important contentions.

When these arguments are kept in view, I think it is clear that my essay says much more than that American culture includes incompatible ideologies of racism, sexism, and liberalism. I also believe that though the various parts of my argument have been suggested to some extent by other scholars, no one has put them all together in this manner, nor has anyone used them to issue the explicit challenges to Tocquevillian/Hartzian accounts that occupy the bulk of my essay. This claim brings us back to point 3, my alleged contention that accounts treating racism as central to American politics are "rare." Since her key criticism is that I wrongly present my view as original, this point is pivotal.

There are certainly many books that insist on the pervasiveness of racism and sexism in American life. There are also many Americans, white and especially black, for whom the reality of white commitments to principles of racial superiority is part of their everyday experience, as are beliefs in gender, ethnic, and religious superiority. My aim was to do something more. I tried to provide an account of American political traditions that directly and explicitly corrected all too widespread academic depictions that either failed to encompass these realities of many Americans' experience or wrongly characterized their relationship to other American values. As I showed by canvassing a number of contemporary scholars, it is "rare" even for academic works focused on inegalitarian doctrines to contend that racism, for example, cannot be fully understood either as just a form of liberalism or as an ugly byproduct of liberal capitalism and liberal ideology (1993, 554–59). Among Critical Race Studies scholars, only Kimberlé Crenshaw has, in my view, clearly made an argument like this; hence I singled out her article.[11] Patricia Williams (1991) has sold many books, true enough; but *Alchemy of Race and Rights* simply does not address the relationship of "racism" and "liberalism" as scholars in the Hartzian tradition have portrayed it.

Other leading figures in Critical Race Studies do treat racism's relationship to liberalism but in a clearly Hartzian manner. Derrick Bell's justly famous casebook on racism in American law, for example, accepts Myrdal's claim that the "American creed" has been violated by white Americans not because of any rival "values and morals" but out of "white self-interest." Bell does depart from Myrdal's optimism on the future of race relations, but not because he sees racism as a principled American ideological tradition. Rather, he embraces "contemporary Marxist accounts" and kindred analyses that stress the enduring "functional utility of racism within a capitalist economy," despite its *violation* of American ideals. Hence, rather than highlighting tensions between American racist ideals and American ideals, Bell endorses Jennifer Hochschild's very Hartzian formulation. Racial problems reflect a "fundamental problem of reconciling liberalism with democracy," a tension between the self-interested desires of the white democratic majority and principled liberal commitments to minority rights. Ironically, in so doing, even Bell manages to describe America's "fundamental problem" in terms that do not explicitly mention racism (1992, 46–49, 60–61).[12]

Another leader in Critical Race Studies, Richard Delgado, has recently cited both Tocqueville and Myrdal on behalf of the view that in America, the nation's "higher, official" values endorse racial equality, while racism is manifested chiefly in informal contexts. The protagonist of his dialogues then argues, in good Hartzian fashion, that it is Enlightenment values favoring the "light" over the "dark" that have spawned racism. Thus "liberal democracy and racial subordination go hand in hand, like the sun, moon, and stars" (Delgado 1994, 726–30, 737, 756). All these formulations reiterate older narratives in which racism is not an American ideology in its own right but just a grim consequence of the nation's core liberalism.

The Hartzian cast of the work of these prominent scholars is far from unusual. Since my essay appeared, Charles Mills has similarly contended that although most of his fellow "black philosophers recognize that white supremacy exists," they "have not, for the most part, constructed it as a theoretical object, a political system in its own right." He argues, much as I do, that "the normative moral and political theories, and the juridical rules, which assign moral standing to, and codify the legal status of, the inhabitants of this society, cannot be understood in terms of an abstract liberalism, even a flawed one. Instead they must be seen as the organic outcome of this peculiar hybrid system," a "white-supremacist liberalism" in which both components are deeply constitutive (Mills 1994, 863).

But though in recent scholarship the article by Crenshaw (1988) I noted and the more recent essay by Mills (1994) come closest to my view, both deal only with the relationship of liberalism to racism, not with inegalitarian ascriptive ideologies generally. Moreover, neither they nor any of the writers Stevens alludes to overtly criticize the Tocquevillian accounts of American culture that are my essay's central target. Therefore, they do not specify how their views differ from such accounts. Those absences were the point of my claim that writers in Critical Race Studies have not offered "explicit critiques of Tocquevillian frameworks" nor elaborated their views as "alternatives" to them. Perhaps Stevens believes the critical implications of their work for Tocqueville, Hartz, and Myrdal are so clear as to make such elaboration unnecessary. But much of her essay makes me think my criticisms need, if anything, to be made more fully, for she provides repeated proof that their implications escape her.

She objects, for example, to my criticism of Michael Rogin by insisting that he does not contend that liberal egalitarian values are primary in America and that his fine 1975 book did not make "world historical claims" but addressed only "the specific role liberalism played in the Westward expansion of White settlers." However, this American analysis is what I criticized, and it does present liberalism as the nation's ideological core. Rogin's opening chapter, "Liberal Society and the Indian Question," characterizes the United States (and also "the modern age") in terms explicitly derived from Tocqueville and Hartz. Rogin writes that "liberalism—to identify the modern impulse by its name in political thought—transformed European societies"; but it "operated on a state of nature in America," because "Americans were, in Tocqueville's phrase, 'born equal'." Thus "the Europeans who settled America were confronted with no alternatives to liberal uniformity save the psychically charged presences of the 'black race within our bosom [and] the red on our borders. . . . Nevertheless, the country lacked the historical bases for political alternatives to liberalism. . . . Liberalism reached everywhere in white America; the resistance it encountered came from within"—in support of which familiar propositions, Rogin cites familiar authorities: Tocqueville and Hartz (1975, 7–8 and 319, n. 20). Even so, Stevens apparently would have us believe that all this really represents an implicit rejection of Hartzian claims for the primacy of liberal values in America. Perhaps obtusely, I still read it as a loyal rehearsal of them.[13]

It is also not reassuring when, after maintaining that my primary flaw is lack of originality, Stevens claims I "overstate" the case by saying that "an array of . . . fixed, ascriptive systems of unequal status" was "largely unchallenged by the American revolutionaries." I meant, especially, that the national revolutionary leadership made no serious official efforts to end slavery or the second-class status of free blacks and women. I stand by that claim. The fact that a few like Jefferson tried unsuccessfully to include opposition to the international slave trade in the Declaration of Independence (while still believing blacks inferior) does not mean that the revolutionary leadership as a whole can be credited with challenging the institution where it was entrenched. Certainly the Articles of Confederation and the Constitution they went on to adopt did not. And it is, to put it very mildly, quite a stretch to present Abigail Adams' letter to her husband as evidence that revolutionary leaders in positions of power seriously considered transforming the status of all those to whom they were denying equal freedom. Stevens is urging us to focus on failed efforts to dispute illiberal institutions in the revolutionary era, rather than the massive official support for those institutions. Her admonitions read like weak attempts to restore, not challenge, Hartzian portraits of early America.

Stevens' primary piece of evidence against me, however, is W. E. B. Du Bois's (1940) wonderful essay, "The White World."[14] I wish I had thought to cite this piece, which indeed "foreshadows the spirit" of my project. That is all it does, however; it cannot do the work that Stevens is trying to make it do. Du Bois tells an anecdote about a white friend who embraced four codes, "Christian, Gentleman, American, White Man," which were in many ways contradictory; nor did the white friend privilege any one of them over the others consistently. The parallels to my argument are real; but it is simply not possible to read these four codes as identical to my map of *liberal egalitarian, democratic republican,* and *inegalitarian, ascriptive Americanist* traditions—a point Stevens reluctantly concedes. Showing these parallels, moreover, is a long way from arguing that this essay by Du Bois provides the critique of Tocquevillian/Hartzian/Myrdalian accounts that my article emphasizes or the explanations of the linkages of the ideological traditions and the ensuing patterns of reform and reaction in American politics that I advance. In his voluminous writings, Du Bois refers to Tocqueville only occasionally but always favorably, and neither he nor anyone else Stevens cites makes anything like my critique of Myrdal.

One might think any reader of "The White World" would make that critique anyway, rendering my argument superfluous. The best evidence that this is not so is provided by Stevens herself. She contends that Myrdal's *American Dilemma* was heavily influenced by this essay and Du Bois in general, a point I do not question. Yet as I argued at some length, Myrdal nonetheless claimed that Americans' higher values embraced freedom and equality for all while denouncing "differences made on account of 'race, creed or color' " (Smith 1993, 551). He contended that Americans saw inegalitarian ascriptive beliefs as "irrational" biases that they had trouble shedding, not elements of an equal but opposing "code." If Myrdal relied as heavily on Du Bois as Stevens contends, then Myrdal apparently did not grasp Du Bois's implicit rejection of such "higher/lower values" formulations. And if Stevens, who does not directly dispute my critique of Myrdal, can nonetheless invoke Myrdal as properly relying on Du Bois, then I must suspect that Stevens has not grasped my explicit rejection of Myrdal's "higher/lower" formulation.

Thus, far from sustaining her claim that many have said everything my article said, Stevens' critique reveals considerable resistance to perceiving, much less accepting, some of its central contentions. Since Stevens is an accomplished scholar, I can only presume that she has repeatedly misrepresented some of my arguments and ignored others because of her overwhelming sense of the importance of the writers she wishes to call to our attention. Again, I commend that sense; and if the upshot of this exchange is that many readers turn to Du Bois, Williams, Rogin, and others to see if they have really said all I have said and said it better, then I will regard Stevens' critique, despite its errors, as worthwhile.

ROGERS SMITH

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Notes

Stevens wants to thank Eileen McDonagh for organizing a roundtable discussion on Rogers Smith's article. She also thanks Cathy Cohen and Don Herzog for comments on an earlier draft. Smith's thanks go to Cathy Cohen, Andrew Koppelman, Adolph Reed, Jr., Ian Shapiro, and Mary Summers for comments on earlier drafts of his response.

1. Smith, in reply to this criticism, claims that by discussing "ascriptive inegalitarian" ideologies his is a broader account than, and hence different from, those of the particular dynamics of "racism" and "sexism"; specifically, Smith adds the categories of "nativism" and the "doctrines of inherent religious superiority," as well as "homophobia" (Smith 1994). My own belief is that at root "nativism" is a form of racism (Balibar and Wallerstein 1991) and "homophobia" (not mentioned in his article) a manifestation of the sex/gender system (Rubin 1975). The inclusion of beliefs in inherent religious superiority as doctrines of ascriptive inequality baffles me, since Smith is surely aware of the Christian abolitionists in the United States and England who used religion against ascriptive inegalitarianism (Jordan 1968, 361–62, 364)—not to mention the suffragists who did the same (Greene 1983). It is true that many Protestants thought themselves superior, but the relation between this and ascriptive inegalitarianism is not as straightforward as Smith implies.

2. Adolph Reed, Jr. (Smith's source for his Lamarckianism claim) believes that Du Bois' thoughts on race changed and that "by 1904 he had begun revising his thinking about race in ways that were incompatible with the neo-Lamarckian resonances surrounding the double-consciousness idea" (1992, 137). Smith's own source on this point suggests that Du Bois did *not* hold Lamarckian assumptions for most of his intellectual career. Also, Reed's article does not make the inferences about a hereditary "Talented Tenth" that Smith ascribes to it. I disagree, by the way, with Reed's identification in Du Bois of an early Lamarckianism, followed by a break in his thought. The work up to 1904 and subsequently can be understood in its entirety as expressing Hegelian insights (and Hegelian confusions) about the ways that *Geist* works through nature, as well as through the state, to institutionalize a Black *Volk*. On the Hegelian strains in Du Bois, see Gilroy 1993; Lewis 1993; Williamson 1984.

3. My source for this claim is the index to *An American Dilemma* (Myrdal 1944). In addition, Myrdal includes several references to the "recent autobiography *Dusk of Dawn*" (p. 601). Ironically, Du Bois was unable to complete his Encyclopedia of the Negro project because of the Carnegie Endowment's commitment of funds to Myrdal's work (Jackson 1990, 265, 429). On the reaction among various groups to Myrdal's study, see also David Southern's (1987) *Gunnar Myrdal and Black-White Relations*.

4. Readers interested in Du Bois's views of America's contradictory traditions of democracy and racism might also turn to Du Bois (*The Suppression of the African Slave-Trade*), [1896] 1973, 198–99; idem 1982 ("The Present Outlook for the Dark Races of Man"), [1900] 1982, 75–78, in which Du Bois accounts for the backsliding of egalitarian reforms (see Smith 1993, 550); idem 1975a (*The Gift of Black Folk*) [1924] 1975, 139, 258.

5. Smith misrepresents Rogin, who does not claim that " 'liberal egalitarian' values are primary in America" (Smith 1993, 556). Rather, Rogin writes, "Jackson's negative, *laissez-faire*, paternal state made the logical marriage of paternal authority to liberal egalitarianism" (1975, 279). Rogin does not claim that *either* is "primary" on the page that Smith cites or anywhere else in the book, to the best of my recollection. Also, Rogin is not making world-historic claims about liberalism or expansionist ideologies. Rather, *Fathers and Children: Andrew Jackson and the Subjugation of the American Indian* offers a theoretically informed account of selected aspects of Jackson's life and times. Thus Smith's observation that "colonial British Americans pursued practices of racial and gender domination long before they embraced the types of liberal republican ideologies and institutions that came to play prominent roles in America" (1993, 556) does not undercut Rogin's thesis about the specific role that liberalism played in the Westward expansion of white settlers, for instance, during the reign of Andrew Jackson.

6. For a similar point about French racism in the aftermath of the Revolution, see Barzun 1965.

7. Among the most established writers in this field are Derrick Bell at New York University, Angela Harris at Boalt Hall, Kenneth Karst at UCLA Law School, Mari Matsuda at Georgetown Law School, Mark Tushnet at Georgetown Law School, and Patricia Williams at Columbia University Law School.

8. Stevens complains further that I never explain "the formation of this mainstream" Tocquevillian/Hartzian scholarship. If her concern is simply to indicate that white male elites, including academics, too often pay attention only to themselves, I agree that the problem exists.

Stevens' criticism may also be that I do not explain when and why Hartzian accounts became so influential. I think those sorts of characterizations were far more extensively qualified and contested in American thought prior to World War II and the Cold War than since. In those war eras, as I suggested briefly, most American white elites decided that explicitly racist institutions had become political liabilities (1993, 562–63). Thus they insisted more unanimously that our liberal traditions had always been our only "true traditions." Leftist critics like Hartz (1955), who wished to trace all American ills to a hegemonic liberalism, were only too happy to agree. Only a small subset of conservatives remained willing to articulate racial conceptions of "Americanism." But Brimelow (1995) presents the history of American national identity in ways that resemble my argument, except that he *applauds* racially and ethnically based exclusions. His book suggests that the Right is now feeling freer to advance racially ascriptive "Americanist" conceptions once more.

9. Stevens says that my assertion that "beliefs in inherent religious superiority" can be "doctrines of ascriptive inequality" baffles her, because abolitionists used religion against ascriptive inegalitarianism. True, but slaveholders used religion to justify slavery. Religious doctrines have been invoked across the political spectrum in America, which is all I said: "Religious elements are better seen as bound up with all three of the traditions identified here. When they are taken as grounds for denying citizenship, as in Protestant nativism, they are closest to ascriptive Americanism; but religion is not the only source of Americanism, nor can Americanists claim to be the 'authentic' voices of American religiosity" (1993, 564). I must assume that in reading me as treating religious beliefs only as forms of inegalitarian ascriptive ideology, Stevens overlooked these sentences.

10. Stevens endorses Balibar and Wallerstein's (1991) view that nativism is a form of racism and Rubin's (1975) that homophobia is a manifestation of gender inequalities. I agree; but it remains true that these authors do not stress the common appeal of the whole range of ascriptive inegalitarian traditions that I consider.

11. Though Stevens does not acknowledge it, my essay also discusses the work of Kenneth Karst (1993), whom she identifies with Critical Race Studies.

12. In case this "reconciling liberalism with democracy" formulation makes Bell appear warm toward liberalism, let me add that Bell sees the selfishness of the white majority as driven by a capitalist economy structured in accord with liberal principles. I do not deny economic motives for white racism but think it is not reducible to them; nor has white economic selfishness always been "capitalistic." Bell's posi-

tions here resemble those of the influential African-American historian Barbara Fields, whom, instead of Bell, I chose to critique in my article (1993, 556). Stevens ignores the presence of that critique as well.

13. Stevens also fusses that I wrongly call Rogin a "postmodernist." Rather, I said that his work draws on "postmodernist" arguments concerned with the political and psychological sources of "otherness" (1993, 556). That theme is recurring in Rogin, though his 1975 book explores it via reliance on Freud rather than the more "postmodern" focus on the subversion of oppositions through "doubling" imagery that is a leading motif of his more recent essays (e.g., Rogin 1992).

14. Stevens also taxes me for saying that at the time of his attack on Booker T. Washington, Du Bois embraced neo-Lamarckian views of hereditary racial characteristics. She prefers to see his view then and thereafter as Hegelian. But for Du Bois (as for many other intellectuals in the 1890s), Hegelian and Lamarckian views dovetailed, so one could easily embrace them both. In his essay "The Concept of Race," which precedes "The White World" in *Dusk of Dawn*, moreover, Du Bois (1940) describes himself as initially persuaded by "scientific" evolutionary accounts of racial differences. Later, especially in Germany, Du Bois heard race instead presented as "a question of comparative culture" in Hegelian fashion. But Du Bois describes himself as "in revolt" against these presentations, on his way to seeing racial theories as rationales for economic exploitation (pp. 98–101, 129). Du Bois's description of himself as first more persuaded by late-nineteenth-century evolutionary doctrines and then as later changing his views does not fit Stevens' description of his thought as "in its entirety" expressing "Hegelian insights." For that claim, Stevens cites only secondary sources. In any case, I merely used the Du Bois of the 1890s to exemplify how in this period inegalitarian ascriptive ideas were embraced by a wide range of American thinkers (1993, 562). Despite her critical tone, Stevens does not dispute this general contention.

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NOTES FROM THE MANAGING EDITOR

FORTHCOMING

The following articles, research notes, and controversies have been tentatively scheduled for publication in a forthcoming issue.

Sidney Verba. "The Citizen Respondent: Sample Surveys and American Democracy." 1995 Presidential Address.

David Austen-Smith and Jeffrey S. Banks. "Information Aggregation, Rationality and the Condorcet Jury Theorem."

D. Scott Bennett and Allan C. Stam, III. "The Duration of Interstate Wars, 1816–1985."

Sarah A. Binder. "The Partisan Basis of Procedural Choice: Allocating Parliamentary Rights in the House, 1789–1991."

Daniel P. Carpenter. "Linear Signal Processing and Budgetary Control in Federal Regulation."

Romand Coles. "Liberty, Equality, Receptive Generosity: Neo-Nietzschean Reflections on the Ethics and Politics of Coalition."

Darrell Dobbs. "Family Matters: Aristotle's Appreciation of Women and the Plural Structure of Society."

John Huber. "The Impact of Confidence Votes on Legislative Politics in Parliamentary Systems."

Jeffrey C. Isaac. "A New Guarantee on Earth: Hannah Arendt on Human Dignity and the Politics of Human Rights."

Jonathan N. Katz and Brian R. Sala. "Careerism, Committee Assignments, and the Electoral Connection."

Jeffrey W. Legro. "Culture and Preferences in the International Cooperation Two-Step."

Arend Lijphart. "The Puzzle of Indian Democracy: A Consociational Interpretation."

Amy R. McCready. "The Ethical Individual: An Historical Alternative to Contemporary Conceptions of the Self."

Brian C. Pollins. "Global Political Order, Economic Change and Armed Conflict: Coevolving Systems and the Use of Force."

Mark E. Warren. "Deliberative Democracy and Authority."

Ada W. Finifter. "Attitudes toward Individual Responsibility and Political Reform in the Former Soviet Union: Replication and Analysis of Recent Findings." A Controversy.

Arthur H. Miller, William M. Reisinger, and Vicki L. Hesli. "Understanding Political Change in Post-Soviet Societies: A Further Commentary on Finifter and Mickiewicz." A Controversy.

ERRATA

In "Party Cleavages and Welfare Effort in the American States" by Robert D. Brown, March 1995, there is a problem with the regression equation reported in Table 4 on page 30. Finite solutions for all of the estimated parameters exist only if the equation's constant is constrained to zero; this is impossible with the heteroscedasticity correction as employed. The only solution is to add data. The new data set contains 13 time points (1976–1988), the same time frame used to develop the characterizations of state cleavage structures. The estimator used is a feasible weighted least squares estimator that corrects for the effects of autocorrelation and cross-sectional heteroscedasticity (as found in Kmenta 1986, 618–21), as well as regional (south–nonsouth), heteroscedasticity. The estimated parameters are as follows (with standard errors in parentheses): party control .00087 (.00023); southern party control interaction −.00102 (.00046); post-New Deal party control interaction −.00116 (.00043). All variables are significant at $p < .01$ and all substantive interpretations remain consistent with those in the original publication.

In "The Advantage of Being Moderately Cooperative" by Tomonori Morikawa, John M. Orbell and Audun S. Runde, September 1995, the matrix on page 602 is in error. It should read:

$$\begin{array}{cc} 1,\ 1 & -2,\ 2 \\ 2,-2 & -1,-1. \end{array}$$

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POLITICAL THEORY

Reappraising Political Theory: Revisionist Studies in the History of Political Thought. By Terence Ball. Oxford: Clarendon, 1995. 310p. $59.00 cloth, $19.95 paper.

Terence Ball's new book collects a dozen essays (five previously unpublished) that tackle issues large and small involved in the study of the history of political theory. A companion volume to Ball's *Transforming Political Discourse,* (1988) and to a promised third one on the philosophy and history of social science, this collection argues Ball's case for the continued relevance of such studies to the larger project of political theory. Toward that end, in the first two essays—"Reappraising Political Theory" and "Whither Political Theory?"—Ball approaches broad questions of interpretation and method, while in the remaining essays he presents his own forays into specific theorists or texts in what he suspects many will pejoratively term *the canon.* The persuasiveness of Ball's position therefore hinges on the strength of convergence of these two lines of attack.

Ball insists that what he sees as a pervasive penchant among political theorists to engage in increasingly arcane arguments over methodology is a function of increasing academicization and specialization. In coming to this, theorists have forsaken their "vocation." As Ball wishes to see it, "the historical study of political theory . . . is a problem-solving activity" (p. 29), and the issue of method should be subsumed within the problem being addressed. Further, the problems that are addressed are immediately political ones, since the proper role of political theory (mostly obscured these days by the methodological debates) is "to draw upon all available sources in reviewing, appraising, criticizing, and perchance occasionally appreciating, the arrangements of one's own society" (p. 57). Ball cites the global environmental crisis as an example of an overriding political problem that has been virtually ignored by political theorists, at least in the pages of *Political Theory,* one of the leading journals in the field (and the original publication site for two of these articles). Going through its table of contents, "one might instead infer that there is something called the crisis of 'the constitution of the subject'' or of the 'body' and 'desire'. Strangely self-absorbed crises for strangely self-absorbed times. Amidst real destruction—economic, environmental, ethical—we engage in deconstruction" (p. 54). What Ball calls the tradition of Western political theory is itself "the most valuable source upon which we have to draw" (p. 57) in pursuing theory's proper task of tackling our political "arrangements."

Ball's assertion that his approach is "*pluralist* and *problem-driven*" (p. viii) should thus be understood to be less pluralist than it seems, since he excludes what he variously refers to as deconstruction, postmodernism, or poststructuralism from the group of possible methods even to be taken seriously. Instead, Ball remains within an interpretive framework bounded by questions of intentionality, context, and prior interpretations and so situates his approach in relation to Hans Georg Gadamer, the so-called Cambridge school (e.g., Quentin Skinner, John Dunn), and, to a lesser degree, reception theory. And the essays included here that deal directly with the history of political theory are, in fact, driven much more obviously by problems arising from past interpretations than they are by contemporary political problems—which often emerge, (when they do) suddenly and at the end. In "Machiavelli and Moral Change," for example, Ball makes a persuasive argument that the Florentine's concept of *virtù* should be seen as homeric in inspiration—not Christian or Ciceronian—and thus signals Machiavelli's intention of reviving an archaic moral principle and offering it as a model for emulation. Ball likens such an effort to the travails of Don Quixote, the difference being that Cervantes knows that his character is tilting at windmills, while Machiavelli is apparently ignorant of the "irreversibility of moral change" (p. 71). This part of the interpretation is less persuasive than the first, since it could be argued that the absence of any successful modern prince in Machiavelli's book is evidence at least of the absurdity of the task and perhaps of the author's satiric intentions. And Ball commits an archaism worse than any his subject could be accused of by labeling him "an ardent Italian patriot and proto-nationalist" (p. 76). It is not at all clear to what Machiavelli is referring when he writes "I love my *patria* more than my own soul," though Ball tendentiously and contradictorily renders *patria* as "native city" (p. 76). But these interpretive questions suddenly pale when Ball concludes the essay by asserting that Machiavelli was not alone in believing that archaic moral orders can be restored, lumping him in with Robespierre, Mussolini, Pol Pot, Yukio Mishima, the Ayatollah Khomeni, and the Moral Majority. Perhaps against his own intentions, Ball thus revives the old Machiavelli-as-devil view, only from a modern, rather than Elizabethan, perspective.

Similarly, Ball begins an investigation of the place of civil religion in Rousseau's *Social Contract* by noting the difficulty scholars have had in reconciling the chapter on this subject with the rest of Rousseau's work. Ball also notes that the theme of civil religion is much in evidence today, that Michel Foucault recently resurrected a reading of Rousseau as a theorist of totalitarianism, and that all of these present the problems to be solved. Ball's interpretation of Rousseau is once again subtle and stimulating. He argues that the *Social Contract* should be seen as a text that constitutes a dialogue between author and reader and that the civil religion chapter is a kind of self-examination for the reader. If the reader has learned the language of the citizen/legislator, then he or she will realize that the civil religion is not needed. If not, then the strictures of the civil religion will substitute the "temporary bonds of illusion" for the true wisdom of the legislator. As evidence, Ball notes that Émile has no need of civil religion because his civic education is complete. This is an ingenious interpretation, though it begs the question whether such an education is even conceivable. But in returning, in a "coda," to Foucault's criticism of Rousseau's desire for a "transparent society" (which owes much to Jean Starobinski's study of Rousseau), Ball simply reiterates his own reading of reading as a form of self-examination in rebuttal. If the stakes are at all high in terms of the contemporary debate over civil religion, one would wish for a more sustained confrontation with the Starobinski-Foucault argument. Ball does return to the question of civil religion in an essay on James Mill and Auguste Comte and concludes it with the question—How is a society to socialize its members and legitimize its institutions?" (p. 156)—and notes that this is not seen as a problem by Nietzsche or Foucault. But when he goes on to an analysis of Bentham that seeks to show that Bentham's ideas about punishment should be read in the light of James Mill's Platonism, his main point is that Foucault's notion of genealogy is inadequate for understanding intellectual debts in Western political thinking and thus can distort our readings of moderns theorists. Meanwhile, the assertion that "Mill regards punishment as a kind of remedial moral education" (p. 169) passes without engagement with the obvious Nietzschean or Foucauldian criticisms of such a practice. As a consequence, the larger questions of socialization and legitimization are left standing unchanged.

Other chapters are framed more narrowly. A pair of essays on Marx and Vico and on Marx and Darwin make some important distinctions in response to the assumptions of other interpreters. In the first, Ball shows that the assertion that Marx and Vico mean the same thing when they assert that human beings can understand history because they have "made" it themselves is erroneous: Marx had a productive or technical conception of "making" in mind, while Vico's was communicative and based in language. In the second, Ball is at pains to show that Marx never intended to dedicate part of *Capital* to Darwin and that the two had distinct notions of nature and of evolution. The point here is to destroy the notion that Marx was in any way a Social Darwinist, a contention I would have thought long dead, at least since the study by Alfred Schmidt's *Concept of Nature in Marx* (1971), which Ball cites. "Utilitarianism, Feminism, and the Franchise" offers a thorough investigation of the controversy generated by James Mill's views on women's suffrage and redeems him to some degree by showing that his views on women's oppression in India were the likely source for John Stuart Mill's feminism, rather than the younger Mill's relationship with Harriet Taylor.

The final two essays hold more promise to deliver on the engagement of political theory with contemporary political issues. The first, on constitutional interpretation and "original" intent is perhaps the most successful in this regard. Ball brings his interpretive arsenal to bear on the doctrine of "originalism" and obliterates it by attacking on several fronts, including the fallacy of attempting to recover past moral contexts in rendering contemporary judicial decisions, the error of treating "the Founders" as a single author, the opacity of language, and much else. In a neat twist, Ball enlists Madison in support of his arguments. The last chapter, "The Myth of Adam and American Identity," is an attempt to show the continued mythic character of American identity by unveiling an image of an American Eden in the works of John Rawls, Richard Rorty (whom Ball takes to be representative of postmodernism), and the futurologist Gerard O'Neill. This image fits the mold of the "unencumbered self," and Ball contrasts it with the purveyors of so-called identity politics, who sell "an alternative vision of the eminently encumbered self" (p. 295). But the essay ends with this juxtaposition and the observation that neither view offers a "sustainable civic vision" (p. 296), and one is left with the feeling that another opportunity to bridge the alleged gap between politics and political theory has been lost. And here, too, Ball seems to acknowledge the political importance of the supposedly selfish concerns of "poststructuralism."

In sum, Ball offers often excellent and sometimes stimulating readings of particular theorists and texts, but these readings owe less to what he thinks of as real politics than they do to the academic politics he decries. When one contrasts these essays with, for example, Jacques Derrida's engagement with issues of nationalism, racism, law, and violence and even with the legacy of Marx (all of which evidence the highest respect for the tradition of Western thought), it is hard to accept that other ways of going about the practice of political theory are "strangely self-absorbed."

University of Massachusetts, Amherst NICHOLAS XENOS

Feminist Contentions: A Philosophical Exchange. By Seyla Benhabib, Judith Butler, Drucilla Cornell, and Nancy Fraser. New York: Routledge, 1995. 176p. $45.00 cloth, $14.95 paper.

This volume is the most lively and penetrating introduction I know to the debate on postmodernism in feminist theory. Its bite is interactive, as four contending theorists directly address one another's positions, then return to the fray after absorbing the others' thoughts and criticisms.

The volume begins with Seyla Benhabib's posing feminism and postmodernism as allies in the "struggle against the grand narratives of Western Enlightenment and modernity," which have forced history into a Procrustian bed of unity, homogeneity, and linearity; but in its sweeping condemnation, postmodernism has swept too much into the ash can. She welcomes the "weak versions" of the major postmodern claims and rejects their "strong versions." The weak versions reject hegemonical claims to interpretation and embrace situated selves, while the strong versions reject agency (through the "death of the subject"), a future interest in emancipation (through the "death of history"), and legitimation (through the "death of metaphysics").

Benhabib would keep "the traditional attributes of the subject of the West, like self-reflexivity, the capacity for acting upon principles, rational accountability for one's actions and the ability to project a life-plan into the future" (p. 20), while reformulating these to take account of "the radical situatedness of the subject." In contrast, she charges Judith Butler, among others, with bidding farewell to the "self as the subject of a life-narrative."

Judith Butler then enters her statement. All these strictures against postmodernism, she writes, warn against an impending nihilism and in response try to "establish in advance that any theory of politics requires a subject" (p. 35). But to require the subject (i.e., a "stable subject") means "to foreclose the domain of the political" (i.e., the domain of struggle) and to silence summarily any political contest over the status of that subject.

In a critique of Habermas and Benhabib, Butler argues that we cannot "ground a theory or politics in a speech situation or subject position which is 'universal' " when "the very category of the universal" contains "its own highly ethnocentric biases" (p. 40). "What speaks," she asks tellingly, "when 'I' speak to you?" "I" cannot "preside over the positions that have constituted me, shuffling through them instrumentally, casting some aside, incorporating others. . . . The 'I' who would select between them is always already constituted by them" (p. 42). Agency, she argues, "belongs to a way of thinking about persons as instrumental actors who confront an external political field" (p. 46). If we *presume* agency, we do so "only at the cost of refusing to inquire into its construction." Butler makes recognizing the constituted character of the subject "the very precondition of its agency," for only this recognition makes it possible to rework, resist, reconfigure, and turn against itself the existing structure of cultural and political relations.

Butler then turns to the political necessity within feminism to "speak as and for *women*." This necessity, she writes, needs to be reconciled with the complementary necessity of continually contesting the meaning of that word. Feminists should assume that " 'women' designates an undesignatable field of differences," so that "the very term becomes a site of permanent openness and resignifiability." She urges, on these grounds, that "the rifts among women over the content of the term ought to be safeguarded and prized, indeed, that this constant rifting ought to be affirmed as the ungrounded ground of feminist theory" (p. 50).

Nancy Fraser deftly summarizes the debate, criticizes each theorist for creating "false antitheses and unnecessary polarizations," then poses her own "middle position" on each issue.

Rejecting (with Benhabib and Butler) the history of metanarratives, she argues for large-scale empirical narratives that are both fallibilistic and nonfoundational, in conjunction with local narratives, which can counteract the distorting tendencies of the larger scale, recover lost traditions of resistance, restore historicity to practices and categories earlier misapprehended as natural, revalue previously derogated actions and forms of culture, and reveal the gender subtexts of concepts like "class" and "the state."

Rejecting (with Butler, against Benhabib) the metaphysical presumption that some justificatory metadiscourse can articulate validity criteria for every first-order discourse, she argues for a "*situated* social criticism" that contests and clarifies traditions, interpretations, and social practices in a way that is itself situated. Such a stance can criticize norms by exposing their geneology and appeal to larger general norms while recognizing that these general norms are themselves situated.

Rejecting (with Butler, against Benhabib) the view that critical capacities are " 'ontologically intact', as opposed to culturally constructed," she argues that even culturally constructed subjects are capable of critique. She opposes her word *critique*, with its connotations of warrant, justification and a claim to validity, to Butler's *resignification*, which on its face is normatively neutral.

Fraser then rejects Butler's claim, following Foucault, that whenever someone is authorized to speak, another is necessarily deauthorized. She urges instead the construction of practices, institutions, and forms of life "in which the empowerment of some does not entail the disempowerment of others" (p. 68)—an admirable goal that is in my view often impossible to achieve.

Fraser further argues against Butler that "foundationalist" theories of subjectivity (including those that presume agency) are not inherently oppressive but have had emancipatory or oppressive effects for historically contingent reasons.

Finally, Fraser contends that claims about "women" are inescapable but should always be subject to revision. Against some French feminists (and Drucilla Cornell), she argues that the word *women* need not signify "difference" any more than *workers* or *Chicanos*. Against Butler, she argues that the uncritical celebration of "differences" among women fails to confront the real conflicts of interest within the group.

Drucilla Cornell's chapter complements Butler's in stressing the unending character of the struggle against the "incessant fading" of differences among women in the face of homogenizing cultural representations; the struggle for a respectful and "nonviolent relationship to the other and to otherness in the widest possible sense" (p. 78); and the struggle to reimagine and resymbolize what we are when existing constructs of what we are, deeply embedded in our language, symbolic structures, and psyches, make that reimagining almost impossible. In an implicit criticism of Benhabib and Habermas, she writes that we must try to "make unconscious patterns conscious without by definition knowing that we are succeeding or being authentic. . . . As a result, we cannot develop standards which say 'enough' " (p. 84).

Cornell develops her argument via certain key concepts in the psychoanalytic work of Jacques Lacan. Her section, "Why Lacan?" however, convinced me only that Lacan's framework pointed in exactly the wrong direction by insisting that man/Woman relations had a primordial (rather than an arguably archetypical) role in framing the human experience of self and Other.

The next four essays by these authors, based on their interchange, illuminate even more clearly their positions and disagreements. Butler remarks gloomily on "the parochialism of these debates," which originally took place in 1990 and include commentary from 1993 and 1994, pointing out that they omit many issues now salient in feminist theory. That gloom is, in my view, misplaced. These essays provide nuanced treatments of the key positions in an extremely important debate. They also do a brilliant job of sharpening analytically two critical problems for our era: how we conceive of ourselves as acting, responsible agents when the "I" trying to act on an external political field is always already constituted by that field and how we commit ourselves to norms that can trump self-interest when all the norms we have (as well as the language, symbols, and cultural apparatus we have to create new norms) are suffused with meanings that both subordinate us and help us oppress others.

Northwestern University JANE MANSBRIDGE

Interactions: Some Contacts between the Natural Sciences and the Social Sciences. By I. Bernard Cohen. Cambridge: Massachusetts Institute of Technology, 1995. 204p. $16.95 paper.

From Aristotle's biological analogies to recent work in sociobiology and evolutionary psychology, social and political thought has kept close connections with the natural sciences. Many of these encounters have been fruitful, but some are merely fruity: recognizing the difference is not always easy.

In *Interactions: Some Contacts between the Natural Sciences and the Social Sciences,* the eminent historian of science, I. Bernard Cohen, presents succinct, informative accounts of these complex interconnections, especially during the early seventeenth century but also through the nineteenth and into the twentieth century. He also draws a number of general conclusions from the historical studies. The book falls into three distinct parts, with sections previously published elsewhere. Chapter 2, "The Scientific Revolution and the Social Sciences," examines influential seventeenth-century authors who illustrate the interplay of the natural and social sciences during early decades of the scientific revolution, including Grotius, Spinoza, Vauban, Graunt, Petty, Hobbes, and Harrington. Chapter 1, "An Analysis of Interactions between the Natural Sciences and the Social Sciences," ranges widely across centuries and disciplines, focusing mainly on economics and sociology as the basis for more general observations about this interplay. Chapter 3, "A Conversation with Harvey Brooks on the Social Sciences, the Natural Sciences, and Public Policy," reviews efforts of social scientists to gain inclusion in the National Science Foundation, the Committee on Science and Public Policy of the National Academy of Sciences, and the President's Sciences Advisory Committee, with reflections on perceived similarities and differences between the natural and social sciences that affected the success of these efforts.

Social scientists have turned to the natural sciences for two main objects, not clearly separable: as a guide to the discovery of new knowledge and as a source of legitimacy for their own work. In diverse branches of the natural sciences, social scientists have found concepts, models, methods, hypotheses, and even laws that they have appropriated more or less directly (as metaphors, analogies, homologies, or identities) for their own use. At the same time, social scientists have understood that the more their work resembles a natural science (particularly an intellectually ascendant natural science), the more credible are their claims to valid knowledge, public attention, prestige, and funding. In the twentieth century, the most successful social science in this respect has been economics, some of whose leading late-nineteenth-century progenitors, notably Jevons and Walras, looked explicitly to physics not only as a guide to economic knowledge but deliberately as a source of legitimation.

Fruitful interactions among the natural and social sciences have not been a one-way or single-lane street. Sometimes developments in the social sciences (e.g., probability statistics) have been adapted in the natural sciences (Darwin's debt to Malthus is another important instance), and sometimes borrowings have occurred between particular branches of the natural or social sciences. Appropriations during recent decades into political science from economics and psychology are obvious examples. Although a number of different branches of the natural sciences have been promising sources for the social sciences, the most enduring has been mathematics. From Hobbes's paean to geometry through Riker's adaptation of game theory to more recent applications of chaos theory, mathematics has been a fertile field for political scientists and other social scientists.

Natural, as well as social, scientists have shared the optimistic, sometimes euphoric belief that the adoption of methods and theories from the natural sciences could overcome the epistemological and practical limitations of the social sciences. In the recent history of our own discipline, the behavioral revolution was such a time of great expectations but with an important twist. Unlike other encounters when the impetus came from particular branches of a natural science (Newtonian mechanics, Harveyan physiology, or Darwinian evolution), the behavioral revolution was inspired less by a branch of natural science than by the philosophy of science. Not Einstein or Morgan but Nagel, Hempel, and other philosophers were guiding lights. As a result, much attention was devoted to general epistemological and methodological issues, rather than the specific contributions of a particular natural science. This opened the door to an eclectic (not to say, promiscuous) appropriation of concepts, models, and methods from a broad variety of sources, all in the name of becoming scientific.

Interactions between the natural and social sciences have been ongoing for centuries, but not all have been fruitful. Does the history of these interactions offer clues to the more promising encounters? As a general principle, Cohen affirms the autonomy of sciences. Beyond that, the proof is in the pudding: "The emulation of the natural sciences by a social science carries with it a validation of the methods used and a legitimation of the enterprise in question. . . . In the end, however, the worth of the result will not be gauged by its resemblance to, or even direct kinship with, one or another of the natural sciences so much as by the degree to which it serves its own discipline or by its applicability to the solution of some practical problem" (p. 72). The degree to which importations from another field serve a particular science is, in the end, a judgment of the community of practitioners, which is not, of course, infallible. Scientific communities are not immune to collective delusions. The term *science* itself has an interesting history of changing meanings, which Cohen briefly sketches. In this respect, the natural and social sciences are in a similar situation. But with respect to the applicability of new formulations to "the solution of some practical problem," the natural and social sciences diverge sharply. The "solutions" based on the natural sciences (practical technologies or types of engineering) work or do not work far less ambiguously than the "solutions" based on the social sciences (mainly institutional structures and public policies). The interactions between natural sciences and practical technologies, like the interactions between social sciences and public policies, have histories that are no less illuminating than the interactions between the natural and social sciences. In the natural sciences, the long history of such interactions between practical technology and scientific theory has clearly worked to the mutual benefit of both. In the social sciences, practical demonstrations of valid knowledge are never so straightforward. Economic policies are a case in point. The most "scientific" of the social sciences cannot rely upon unequivocal practical corroboration of its theoretical claims. Economic policies have, at best, temporally limited success, and the extent of their success is always subject to controversy. For political scientists, the difficulty is greater. The point is that social scientists derive far less benefit than do natural scientists from practical applications in providing independent corroboration of their knowledge, which is one reason why contradictory theories can coexist indefinitely.

A general lesson from Cohen's instructive review of interactions between the natural and social sciences is that we should be tolerant of efforts to incorporate contributions from other fields, even if they are sometimes ill conceived. The growth of knowledge is a recondite process, nourished at times from unlikely sources. These efforts may not succeed, and they may proceed from questionable motives; but there is always a chance that light will be shed from unexpected quarters.

University of Minnesota, Twin Cities EDWIN FOGELMAN

From Art to Politics: How Artistic Creations Shape Political Conceptions. By Murray Edelman. Chicago: University of Chicago Press, 1995. 152p. $18.95.

Postmodern artists, symbiotically nurtured by their partners in academia and criticism, are nothing if not politically relevant. More effort is invested in the explanatory concordances of exhibited work than in the work itself. And the artwork often reinforces catalogue narratives by appropriating the essay motif. Disturbed by the hypocrisies of contemporary life and doubly so with modernist escapism, postmodern artists hope to communicate directly, clearly, and forcefully.

According to Murray Edelman, their chance of success may be pretty good. Art, he claims, sets the agenda for politics. While political language is process-oriented and unequipped to consider "outcomes," the language of art punctures political hype and parochialism to focus on destinations, rather than means of transportation. Politics seeks support, not understanding, and is thus willing to falsify the depiction of outcomes for the sake of expediency and power. Art, according to Edelman, embraces complexity and ambiguity, conditioning minds for tolerance and experimentation and offering a haven from deadly routine.

However, art also serves the interests of the status quo, sacrificing its disruptive potential to reactionary interests. How and why artists succumb to these interests is not discussed, and artists who read this book will become justifiably offended with this alleged conspiracy: they are either witless dupes or greedy accomplices. In any case, Edelman implicates art in an inventory of contemporary travesties. Art condones the unrepresentative ethnic composition of the American armed forces. Art maintains the humiliation of welfare recipients. Art prolongs the suffocating and demeaning stereotypes of women. Art supplies the whitewash covering American adventurism in the Persian Gulf. Art protects the sham of American elections. And art reinforces the facade of impartiality for bourgeois justice.

So art either supersedes politics or serves politics. To reinforce the quandary, Edelman employs two distinct definitions of art. In his more indignant discussions, like that regarding the symbolic depiction of women, he defines "art and literature" by its components: "novels, paintings, photographs, stories, movies, television dramas and sitcoms, cartoons, off-color jokes, and so on" (p. 46). What Edelman means here by art is really image: "Philosophers and historians define art in many different ways; but for this study, its crucial feature is that it supplies images that construct the worlds in which we act" (p. 3). The problem with a book about the power of images, however, is that it has already been written—with infinitely more power and elegance. Surely, Plato's *Republic* deserves a footnote for statements like "There cannot be any representation that reproduces another entity, scene, or conception, but only constructions that may purport to reproduce reality while simplifying, elaborating, accenting, or otherwise constructing actualities and fantasies" (p. 7). Yet Plato would never have confused the more generic concept of image with the more specific qualities of art. The subset of images that qualify as artistic are especially provocative, complex, and disruptive. Images are to be manipulated, but art is mostly banned.

Edelman seems to recognize the exclusivity of art in his second definition—that which he attaches to discussions regarding art's special perceptiveness. Gone are the references to off-color jokes and sitcoms. Instead, expressions that would easily fit under the first definition, like *kitsch*, are disqualified in the second as "spurious" (p. 33). And socialist realism, because it "reiterates pieties rather than undermining them" is "not art even though it looks like it in form" (pp. 56–57). Instead, "art teaches us to see the world in new ways" (p. 109), "especially through works that are not explicitly political" (p. 43).

So Edelman finally does privilege the imagination, the transcendence, and the sensuality of certain works of art. And he distinguishes, correctly, the ability of these works to inspire their audiences in ways unavailable to more tendentious forms. But is the inspiration of this art political? Edelman admits that audience responses to these more sublime creations are less predictable and less controllable. For this kind of art, "responses, not the intentions, determine meanings" (p. 84).

A discontinuity between responses and intentions may be acceptable and unavoidable in fine art, but does not such a discontinuity distinguish these endeavors from their more conventionally political counterparts? Edelman attempts some damage control by insisting that good art is politically inspirational if not programatic: "Art that portrays anguish and bewilderment, for example, . . . must encourage a politics that reflects or confronts that condition" (p. 55). Bertolt Brecht would be hard pressed to agree with Edelman's imperative. Although his *Threepenny Opera* portrayed the depravity of capitalism, bourgeois audiences left the theatre thoroughly delighted and politically unencouraged. Frustrated at his didactic failure, Brecht responded with the *Threepenny Novel,* a much more tendentious sequel, which was resoundingly ignored.

Edelman concludes by lamenting that "artists pay a high price for their limited measure of autonomy" (p. 145). He would rather see more "efficacious tactics" (p. 146) among the enlightened who see the autonomy as a chimera. Edelman implies that if good artists addressed issues like sexism, racism, and imperialism, the imagination and transcendence of their endeavors could overpower the primitive treatments we now suffer. What Edelman does not fully appreciate is the mutual exclusivity of transcendence and tendentiousness: tactical art is bad art. Lenin recognized the incompatibility when he saw the fruits of his call for artistic tributes to the revolution. The purely tendentious monuments were embarrassing caricatures while the work of accomplished artists was unacceptably ambiguous. Boris Korolev's statue of Bakunin was not removed from its wooden scaffolding until the day the project was dismantled.

For art that is not art, then, like sitcoms and jokes, audience response is for the most part determined by the simplicity and tendentiousness of the medium. But to say that jokes and sitcoms can be political is not much more than saying that the imagery of a stump speech is political. To say that good art is political, however, is much more problematic. Artistic masterpieces do not—indeed cannot—determine their interpretations. Thus, while certain simple images can certainly and predictably demean welfare recipients, bolster the military, and stereotype women, art is much less dependable to effect any such instrumentalities.

Edelman is certainly correct when he says in his preface that the political science community has lent scant attention to the connection between art and politics. But that excuse only goes so far, especially in light of the brilliant treatments of Benjamin, Greenberg, Adorno, and Breton (to name a few). The connection is one that begs for exploration by the methodologies and experience of the discipline. This book does little to inspire that exploration.

Santa Clara University TIMOTHY J. LUKES

The Political Thought of Hannah Arendt. By Michael G. Gottsegen. New York: State University of New York, 1994. 311p. $64.50 cloth, $21.95 paper.

In the 20 years since Hannah Arendt's death, debates over the meaning and significance of her work have come to figure prominently in a broad array of academic journals in the United States and, increasingly, in France and Germany. It is likely that Arendt would have been impatient with this popularity and, particularly, with the insular venue and often esoteric themes of these debates. By turns an engaged intellectual who was moved to write by the political catastrophes of her time and a private person who complained when her writings put her too much in the public eye, Arendt explicitly refused to take part in the metatheoretical debates that were staged by those whom she dismissively termed "professional thinkers." Gottsegen's book is true to Arendt's reluctance to distract herself with scholarly squabbles. It is a close textual reading that takes Arendt on her own terms as a theorist of political action who, though no activist herself, wanted to put words to what she understood to be the "public happiness" that is unique to political participation and thereby to reawaken citizens' taste for the pleasures and responsibilities of democratic politics.

The study is organized by what Gottsegen calls Arendt's *action-ideal,* her effort "to delineate a mode of political action that answers at once to the reality of human difference and to the need to insure an ordered world" (p. 12). Gottsegen presents this ideal as a dialectic, arguing that whereas human difference (in Arendt's words, "plurality") calls for politics to provide an arena for the revelation of individual uniqueness through acts of greatness, the need for order provides a countervailing impulse to contain such disruptive acts for the threat they pose to the continuity and stability of the common world. Gottsegen uses the action-ideal as a hermeneutic device to read Arendt's mature political works, starting with *The Human Condition,* in a way that demonstrates their "organic coherence," a unity that he attributes to Arendt's effort to set forth the ideal and then to reconcile its contradictions (p. 1).

Gottsegen's hermeneutic strategy yields a provocative interpretation of the dense and difficult *On Revolution.* Although typically regarded as Arendt's effort to mobilize a historical contrast to exemplify the public/social distinction she lays out in *The Human Condition,* for Gottsegen, this text marks a subtle shift in Arendt's attention. She turns from the self-revelatory dimension of action that prevails in *The Human Condition* to the countervailing task of creating institutions to found a "stable and enduring polity" (p. 119). This shift, he argues, is evident in her remarks about the U.S. Constitution and the Supreme Court. She regarded the Constitution as "the focal point of an origin-oriented cultural tradition" that records and preserves its founding principles, the Court, as the "living voice of the origin" that ensures that subsequent legislative initiatives will be contained within its bounds (p. 124). Gottsegen argues that this shift reveals to Arendt that the imperatives of her action-ideal are "antithetical, yet equally indispensable" in requiring a space of action that would welcome the spontaneous eruptions of self-display by which each individual manifests his or her inborn uniqueness and also contain such instances for the sake of preserving "world-continuity" (pp. 135–36). Thus, in the first two chapters of the book, Gottsegen uses the device of the action-ideal to set up the contrary moments of a dialectic whose synthesis he presents in the third chapter by extrapolating from Arendt's unfinished writings on judging.

The unfinished "judging" volume of Arendt's *Life of the Mind* has intrigued many of her recent commentators, in part because of her idiosyncratic appropriation of Kant's *Third Critique* and also for her promise to model a kind of principled thinking that would be (to borrow Richard Bernstein's phrase) "beyond objectivism and relativism." These are the elements that attract Gottsegen to these writings. In addition to doing a close textual reading that skillfully distinguishes "Arendt's Kant from Kant's Kant," Gottsegen makes an original contribution to the commentaries on these writings by presenting them as the third term of the action-ideal (p. 188). Gottsegen argues that Arendt's judgment writings are not a retreat from her commitment to the standpoint of action (as Ronald Beiner has claimed) but, rather, that they serve to reconceptualize action in terms of "the process of collective deliberation whereby a policy consensus is to be secured from the midst of a multitude of actors" (p. 202). According to Gottsegen, Arendt now recognizes judgment as a second, contemplative moment of action that mediates between its existential, individuating impulse and the common world.

The original insight here is Gottsegen's claim that this reflective moment is not spectatorial and disengaged but, rather, that it is "comprehended as a vital component of action though never as the whole of it" (p. 203). Just what does this moment of judgment entail? Gottsegen describes it as an instant of conciliation in which the self, the "singular root" of individuation, submits to be guided, but not determined, by a common sense that is defined in purely procedural terms (p. 213). Gottsegen sees this last point as representing an important retreat from *On Revolution,* where Arendt's strong claims about the Constitution suggested a more substantive conception of common sense as an authoritative founding narrative that, in terms of the action-ideal, would tip the balance too far against the existential moment of action. Gottsegen interprets this procedure, which Arendt terms "representative thinking," to involve reaching consensus by means of an exchange of opinion among citizens whose primary commitment is to the common good.

The method Gottsegen employs in this text (close textual study) is well suited to his style of argument. He is a sensitive reader of Arendt's work who often elucidates her concepts and arguments poetically and evidently shares her passion for putting words to elusive political experiences. A question to pose of a study such as this (one organized by a hermeneutic device, rather than by extant debates in secondary literature) is, To what extent does it unfold new aspects of Arendt's work or recast well-known elements in an interesting and provocative way?

Although provocative and original, one drawback to Gottsegen's hermeneutic device, the dialectic of the action-ideal, is that at some points, it becomes a formula and, as such, is more of an imposition on Arendt's work than a point of access into it. For example, Gottsegen's rendering of Arendt's remarks on the Constitution stops just short of casting her as a proponent of "original intent"—a surprising reading given the attention, both critical and celebratory, that Arendt has received for her antifoundationalism. Could it be that Gottsegen overstates the case here to set up for his concluding synthesis? A further drawback to this approach is that it mitigates against an exchange with other scholarly writings on Arendt and contemporary theory more generally that, in some instances, might have enabled Gottsegen to put a finer point on some of his claims. For example, having presented Arendt's conception of judgment as a discursive procedure grounded in common sense and oriented toward reaching consensus, Gottsegen poses the important question whether such a consensual model could do justice to the plurality of needs and opinions of which mass democratic societies are constituted. His exploration of this problem could have been enriched by an engagement with the work of feminist scholars such as Iris Marion Young, Jane Mansbridge, Nancy Fraser, or Seyla Benhabib, each of whom has criticized impartiality and abstract proceduralism precisely for its inadequacy to plural democratic polities.

University of Minnesota LISA J. DISCH

One for All: The Logic of Group Conflict. By Russell Hardin. Princeton: Princeton University Press, 1995. 288p. $24.95.

The newspapers for (27 May 1995) report more than 40 villagers shot and hacked to death by the Tamil Tigers in Eastern Sri Lanka, while in the refugee-packed Bosnian city of Tuzla an air-burst rocket fired by Serb forces killed over 70 children, teenagers, and young adults who were thronging the cafés and enjoying the warm evening. That makes it a worse-than-average day for group conflict—but nowhere near as bad as many days in the past or, no doubt, many more days to come. In *One for All,* Russell Hardin's primary purpose is to seek an explanation of group conflict ranging in form from peaceful political jockeying for group advantage to attempts by one group to exterminate another group or drive it from disputed territory by violence.

Hardin's analysis, which is pitched in rational choice terms, is profoundly depressing; for his conclusion is that there are many situations in which it is in the interest of group members to support, and participate in, the pursuit of the collective advantage of the group at the expense of other groups. Even when this is not so, the phenomenon of group identification can be exploited by political leaders in order to achieve, or hang on to, power and the spoils of office, especially if they can exercise complete control over the media of mass communication. Since Hardin defines "rationality" relatively to the information available to the agent, the group members may thus be led to act rationally in ways that are, in fact, destructive of their common interests. Serbia is an illustration of this possibility.

Depressing as this may be, it is, at any rate, less pessimistic in its implications than the alternative explanation tracing group conflict to "primordial" identities. Hardin points out that it is simply not true that differences (e.g., ethnic, linguistic, religious) always lead to conflict. Nor can we usually explain the lack of conflict by saying that it is held down by repression. This is not to deny that the collapse of political authority (as in Yugoslavia or the Soviet Union) may set the stage for conflict to emerge. But this is because it creates incentives for the pursuit of group advantage that did not exist before. There is, in general, no reason for supposing that members of different groups living peacefully side by side must have been harboring all along the wish to attack their neighbors.

Hardin insists that group identity is never a given and that what matters is identification with a group. (Others may claim that you are a member of some group by blood, but it is still open to you to deny the significance of that.) Why should anybody identify with a group? Hardin suggests that from a rational choice perspective, this is an almost trivially easy question to answer. Identification is the solution (or more precisely the recognition of the solution) of a coordination game—the form of a game that raises fewest problems for collective action because in it, it is advantageous to each member of a group to behave in a certain way on condition that the others do likewise. Generally speaking we are more comfortable with, and can cooperate more readily with, people who speak the same language, laugh at the same jokes, have the same tastes (in food, clothes, music, etc.), and share our overall view of the world. We identify with such people not because we feel we ought to but simply because it suits us.

The theoretical importance of this move can be appreciated in the context of the standard problem of collective action. This is the kind of situation (an *n*-person prisoner's dilemma) in which action by the members of a group is collectively advantageous but individually disadvantageous because each can free-ride on the efforts of others. Hardin's suggestion is that no such problem has to be solved to produce the phenomenon of identification. Actually, getting the group into motion requires the identification to be built on; and that may run into a collective action problem. But Hardin argues that so long as members of the group can be confident of others' acting (another coordination problem), the costs to be anticipated may be low. And group members may be able to coerce one another at low cost into participation.

Much of the book is taken up with the elaboration and illustration of the way in which identifications can be mobilized. Two other themes, however, can be traced. Hardin has for a long time been concerned with the analysis of norms within a rational choice framework, and the discussion of this topic (including a criticism of his excolleague Jon Elster) is independently valuable. Conversely, anybody exclusively interested in group conflict could afford to skim chapter 5, on universalistic norms.

The other theme, which comes to the fore in the last two chapters, is normative (as distinct from the explanation of norms). Hardin at a number of points acknowledges David Hume as his master; but one might say more broadly that it is the spirit of the Scottish Enlightenment that breathes in these pages. The best hope for alleviation of group conflict is seen by Hardin to lie in the creation of economic conditions such that individual efforts pay better than zero-sum (or, far more likely, negative-sum) group efforts. (The lack of such conditions in most of Africa explains the intensity of group conflict there.) Strong group identification he regards as not only dangerous because of its standing potential for predation but also as stultifying personal development. Contrary to the communitarians, his ideal society would be one in which the urbane values of openness and toleration of diversity were celebrated—the values barbarously blown away in the shelling of Sarajevo. Chapter 7 nicely dissects the philosophical folly of contemporary communitarianism, an utterly implausible universal theory that has nothing in common with the bases of evaluation employed by members of actual communities.

In spite of its often somber subject-matter, the book is a pleasure to read—lucid, sharp, and quirky. Who else would illustrate the norm of honor by retelling the preposterously convoluted plot of Verdi's opera *Ernani*?

London School of Economics BRIAN BARRY

Postethnic America: Beyond Multiculturalism. By David A. Hollinger. New York: Basic Books 1995. 210p. $22.00.

David Hollinger argues that multiculturalism has understandably risen in response to the gap between the promise of equality to all Americans regardless of color, and the reality of inequality that has persisted for some racial and ethnic groups. Yet it is now time to move past multiculturalism, Hollinger says, to what he calls postethnic America. The problems with multiculturalism are several. Multiculturalism sometimes uses as central categories of analysis a classification scheme developed by the Census Bureau for affirmative action purposes. This "ethno-racial pentagon" consists of (1) Euro-Americans, (2) Asian Americans, (3) African Americans, (4) hispanics, and (5) indigenous peoples. Multiculturalists then reify this pentagon into racial and cultural categories. There is no one Asian culture, however, and there is certainly no one Asian American culture; the same holds true for the other four categories. Hollinger is right to point out that race does not map onto culture, but he could have been more attentive to the interaction between prejudice and cultural practices that sometimes does exist. Africans who were dragged here came from many different cultural backgrounds, but their common experience here as slaves helped them form a new African American culture.

Hollinger also notes that it is a mistake to think of these Census Bureau groups in racial terms. Scientists have exploded the idea that the human species can be neatly divided into several races; history has shown that some groups, like Jews, were once thought of as racial groups but are so no longer. The change had to do with social and political, not biological, causes. The pentagon of groups is simply too rigid to describe Americans. It also restricts the ways in which people can shape their identity, or (as Hollinger would prefer to say) the way they affiliate. Hollinger points out that there are an increasing number of mixed marriages in the United States. The offspring of these marriages often cannot fit neatly into one racial or cultural category. Hollinger contends that the term *race* now does more harm than good and suggests that we replace it with the admittedly clumsy term *ethno-racial bloc*.

The offspring of mixed marriages are increasingly claiming to be recognized as their own, unique groups. This illustrates another problem with multiculturalism, the diversification of diversity. Groups can always subdivide further, to the point where the legal and political claims made by groups becomes absurd. As groups proliferate, as many people want to jump on the diversity bandwagon, an important paradox emerges: while we have witnessed the "basic triumph of the basic multiculutralist idea," so many people claim the label that they have "rendered the word 'multiculturalist' a shibboleth" (pp. 81–82). Unfortunately, Hollinger does not explore the paradox of how a concept that has many different meanings can triumph in a harmful way; one might think that such a concept would be only superficially influential. Nonetheless, I think that Hollinger might be on to something here. He could have sharpened his argument if he cited more than a couple of the multiculturalists he wants to criticize. Similarly, Hollinger does not mention at all two of the more important books in the field, Will Kymlicka's *Liberalism, Community, and Culture* and Iris Young's *Justice and the Politics of Difference*. Some attention to these books would have given Hollinger's argument more focus.

The way past multiculturalism lies in *postethnicity*, which is based on "rooted cosmopolitanism." Hollinger makes a useful distinction among what he calls pluralists, universalists, and cosmopolitanists. Pluralists want to protect existing cultures, universalists want to discover what is (or should be) true for all cultures, while cosmopolitanists are eager to critically explore and learn from other cultures. Hollinger wants us to affiliate with groups that we choose; he wants us to be able to identify with all of our heritages if we want. Postethnicity "appreciates multiple identities, pushes for communities of wide scope," while it recognizes that ethno-racial groups are socially constructed, and that new groups will form in a democratic society (p. 116). Hollinger does not want people assigned to ascribed groups; he wants people to shape their own identities. Many people who already lean toward liberalism will find this familiar idea rather agreeable.

Related to postethnicity is Hollinger's call for a civic nation, instead of one based on ethnicity. The chorus for a civic nationalism, in which all members of a state can join regardless of their ethnic heritage, has become understandably louder and more persuasive in recent years. Hollinger wants to show that multiculturalists who refuse to see that Americans are bound together are making a mistake. The United States is not simply a container for other cultures, nor should it be. While Hollinger is careful to note the prejudice that has harmed the United States, he also points out that few countries are as welcoming to immigrants as the United States. When these immigrants come to the United States, they contribute to, and become a part of, the larger national culture. Hollinger urges us to recognize the importance of the larger, national culture (which can coexist with smaller publics) because it is the only way that we will be able to face our common problems together. Hollinger here fails to note how the problem of language challenges civic nationalism. Since many ethnic and racial groups identify with a language, the language(s) of a state often matters a lot, and no state can have a limitless number of official languages. A glance northward to Quebec illustrates the problems language can cause. Still, a civic nation is a better alternative to the bloody ethnic nationalism of other parts of the world.

Hollinger's liberal response to multiculturalism will undoubtedly find a sympathetic audience among those frustrated both with multiculturalists and the inequality that is pervasive in our society. Yet the basic challenge to Hollinger's hopeful vision—prejudice and racism—is alive and all too well. As he notes, it was prejudice that spurred on multiculturalism. It may be that the best way to ensure that multiculturalism fades is to end racism. However, liberals like Hollinger have not come up with a good way to rid the country of racism. Unfortunately, no one else has either.

University of Nebraska JEFF SPINNER-HALEV

Passions and Constraint: On the Theory of Liberal Democracy. By Stephen Holmes. Chicago: University of Chicago Press, 1995. 350p. $29.95.

Holmes rides again. This time, Stephen Holmes is after libertarians for obscuring the richness of liberal theories of human nature, left-wing critics for alleging that liberal theory has an antiwelfare bias, and both for misunderstanding how liberal constitutional arrangements foster deliberative democracy.

This battle on two fronts is more constructive than Holmes's earlier polemic, *The Anatomy of Antiliberalism* (1993). The arguments in *Passions and Constraint* are capable of standing on their own. They have to, since relatively little space is devoted to the specific arguments of the libertarians or leftists who are mentioned. Even though chapters 2–8 are all previously published essays, they are framed by two introductory essays and a conclusion to form a unified reading of liberal theory that is confident and clear. Whether it is convincing is another matter.

Holmes argues that classical liberal theorists did not have much faith in individuals as rational calculators of their own self-interest. "Bentham aside," the liberal tradition of Locke, Smith, Hume, Madison, and Mill "assumed, quite realistically, that passions could crowd out intersts" (p. 43). These passions were not limited to the "tame" passion for material comfort but included more volatile, even vengeful and malicious feelings and habits. Liberal theorists knew that "people sink below self-interest just as often as they soar above it—perhaps more often" (p. 59). Why add Hobbes? *Behemoth* shows that despite his reputation as a precursor to modern game theory, Hobbes was well aware of the complex role that passions, parochial norms, and religious beliefs play in the constitution of political power.

Bodin, another "preliberal," anticipated the other half of the liberal equation for constitutional government. Bodin realized that limited sovereignty, if limited in the right way, could be more powerful than absolute sovereignty. Sovereigns need the cooperation of their subjects, and sometimes this can be manufactured by tolerating their religious beliefs and avoiding "irresolvable disputes" (p. 131). Similar techniques for enhancing state power, what Holmes calls "positive constitutionalism," are applied by later liberal theorists to the majoritarian liberal state.

To meet the challenge of irrational passions, habits, and behavior, liberal theorists have argued that constitutional arrangements can perform two complementary functions. Not only can they protect the interests of individual citizens from state power, but they can also organize state power to enable citizens to participate more rationally in democratic politics so that they can express these interests and realize them cooperatively. Constitutional arrangements can "bind the hands" and "tie the tongues" of democratic citizens so that, like Ulysses bound to the mast, they can avoid some mistakes and correct others. *Precommitments* (e.g., making constitutional amendments more difficult to pass than normal legislation, separating the powers of government, and fixing in advance the schedule of elections) make it possible for citizens to participate more deliberatively in democracy. They create a sensitive and responsive, not just a fragile, balance of powers. This "collective self-binding" is not only a Schellingian "solution" to the problem of collective action; it is also a way of "encouraging future citizens to think for themselves" (pp. 173–74). Republicans who have criticized liberalism for being antidemocratic—starting with Rousseau, Paine, and Jefferson—have failed to grasp this "paradox of democracy." In contrast, Mill designed the division of labor in his theory of representative government to limit freedom of expression for the sake of greater, more informed public deliberation.

What divisive issues, if any, should be kept off the political agenda for the sake of deliberative democracy? In chapter 7, Holmes takes up several issues including slavery, the separation of church and state, and abortion to show that there are many ways to remove such issues and that gag rules (like precommitments) should be used not just to keep the peace but primarily "to make public debate more rational" and "reinforce democracy" (p. 11). Then, in chapter 8, Holmes sketches the interdependence of deliberative democracy, economic well-being, and education to show how liberal self-reliance is neither callously individualistic nor blind to the dangers of economic inequality. The "paternalism" that liberal theorists have favored has been humane and "autonomy enabling" (p. 265).

I have two criticisms of Holmes's theory of "positive constitutionalism." First, despite his own recognition that past liberal theorists developed their institutional views in order to come to grips with specific historical problems, Holmes does not pursue his own theory to conclusion. Why, as Holmes suggests, should divisive religious issues such as the creationist-evolutionist debate be walled off from majoritarian politics (p. 231) when equally divisive race-related issues like slavery should not? (p. 218). Is it just a matter of strategic success, an "unloading tactic" (p. 206)? If so, then more needs to be said about how precommitments and gag rules create cooling-off periods, rather than fuel resentment. If it is more than a strategic question, then how are competing moral values to be weighed? Slavery, Holmes says, was "sinful" (p. 217). But he also says that postponing full debate may have been the right thing to do in retrospect (p. 222). Should the same be said about abortion? Holmes cannot seem to decide, although he hints abortion may be more like slavery than like religion (p. 229).

A better approach to these divisive issues, I would argue, is through something like what Michael Perry calls "ecumenical political dialogue" and the virtues that citizens acquire as they participate in it. Holmes rejects this kind of republicanism; but it is not clear to me that his "positive constitutionalism" is anything more than a loose reconstruction after the fact of "metaconstraints" (p. 162) on rules of public debate from the external point of view of a Hamiltonian observer.

Second, Holmes's concentration on Elsterian paradoxes leads him to ignore the complex ways in which liberal theorists have actually engaged the political problems of their own times. He touches on this in the chapter on Hobbes when he recognizes that Hobbes was as concerned with the transformation of human passions as he was with the sharpening of human wit to cope with unruly passions. The same was true of Mill. Before representative institutions could work, they would have to appeal to people, not as they were but as Mill thought they could be. To move them in that direction, Mill presented them with a set of related portraits of sound political character he thought they could emulate. Unlike Hamilton, whose model of constitutional democracy is the inspiration for Holmes's title, Mill realized that political character could not just be constrained. It had to be educated, especially since citizens were demanding to play at least a more active role in politics than Hamilton thought they should.

Michigan State University STEPHEN L. ESQUITH

The Autocritique of Enlightenment: Rousseau and the Philosophes. By Mark Hulliung. Cambridge: Harvard University Press, 1994. 293p. $45.00.

We misinterpret both Rousseau and the whole movement of the Enlightenment for want of understanding the relationship between the two. Previous scholars have either made Rousseau a member of the Enlightenment and ignored his critique of it or emphasized his critique but removed him from the Enlightenment. So argues Mark Hulliung, who seeks to correct past misunderstandings by showing that Rousseau belongs squarely within the Enlightenment and that his critique therefore constitutes the autocritique—the immanent, dialectical self-criticism—of the European Enlightenment.

Although Hulliung's point is not so altogether new as he claims, his fine book is the first full-length history of this crucial, internal debate between Rousseau and the philosophes. Dedicated to his teacher, Judith Shklar, this book has some of the flavor of her work. Intuitive with precision, written in a pointed, brisk, confident style, it paints portraits through the rapid accumulation of remarks and observations. It is well researched and copiously annotated.

The book's "basic finding," namely, that Rousseau, despite his radical critique, "never for a moment left the Enlightenment" (p. 242), seems to mean three things. First, Rousseau shared the starting point or basic premises of the Enlightenment: the intention to be useful to humanity, Lockean epistemology, theoretical individualism, the doctrine of rights and social contract, anticlericalism, the method of philosophical history, the hope to use and extend both the procedures and findings of modern natural science.

Second, Rousseau shared with the Enlightenment not only his starting point but also his endpoint or final goals, because "he remained staunchly loyal to the ideals of freedom, individual autonomy, and toleration that typified the 'century of philosophy' " (p. 242). Thus, Rousseau's radical critique was always about the proper "route to enlightenment" (p. 3)—about means, not ultimate ends.

Third, in his criticisms of the Enlightenment, Rousseau was often only stating or radicalizing the philosophes' own self-doubts—their autocritique. An ardent republican, Rousseau attacked the philosophes' praise or appeasement of monarchy; but Voltaire, Diderot, and especially Helvétius, Holbach, and Turgot were all quietly moving in a republican direction (pp. 108–25). Rousseau denounced the decadent alliance of Enlightenment high culture and Parisian high society, recommending rustic retreat, as well as withdrawal into the bosom of the intimate, child-centered, sentimental family; but similar themes can be found in Diderot and especially Holbach (pp. 137–55). Again, Rousseau criticized the philosophes' whole conception of themselves as a new breed of philosophers, as disinterested agents of public enlightenment who were attached to, and engaged in, this world while remaining above its passions and illusions. He showed that their immersion in the world undermined their spiritual independence and gave them as many prejudices and partisan interests as any other group within society. But Alembert and Diderot (most strikingly in *Rameau's Nephew*) entertained similar fears (pp. 76–108). In all these cases, Rousseau's position was an extension of, as well as an attack upon, the philosophes' views.

Hulliung's book is most interesting not in making the first of these points (which everyone more or less admits), nor the second (which seems to me problematic), but the third: in accordance with its announced intention, it teaches more about the Enlightenment than about Rousseau. Regarding the latter, it presents an able statement of more or less available views; but in treating the philosophes, it breaks new ground, giving us a much broader, more complex view that highlights the inner disagreements, the churning self-doubts and revisions, the autocritique of the Enlightenment.

Furthermore, an adequate understanding of this self-criticism is vitally important because it remains very much alive today. Just as the battle between the philosophes and the Church has continued in some manner to the present time, so the conflict between the Enlightenment and some form of self-critical, philosophical counter-Enlightenment has asserted itself in every generation since Rousseau. Virtually from the beginning, the modern world has been a three-sided battlefield; and the Enlightenment rationalist of today, no less than Voltaire and Diderot, must struggle against both the religious camp and the philosophical critics of reason, whether romantic, existentialist or postmodernist. Why the Enlightenment has been so autocritical and self-devouring is one of the great questions of the age; and this book, by taking us back to the beginning of this phenomenon, helps us to think about it.

Judged from this highest standpoint, however, the book also has some shortcomings. Primarily a work of intellectual history, it tends to report the arguments of its protagonists, rather than to engage, grapple with, and evaluate them. Thus, in making the important point that Rousseau started from the same basic premises as the philosophes, it does not go on to analyze whether the very different conclusions he derived were correct—indeed, whether they somehow represent a more rigorous thinking-through of the shared premises of, and thus a necessary dialectical advance beyond, the Enlightenment view. Hulliung, in other words, stops tantalizingly short before the big question: Is the "autocritique" that is his theme a mere matter of revisions and changes of mind, or does it ultimately bespeak some fundamental and identifiable inconsistency within the Enlightenment position that necessarily forces it beyond itself? Is this the fundamental reason why the Enlightenment has always generated its antithesis?

A different possibility is that Rousseau and others criticize the Enlightenment not primarily from within, by thinking through its premises more consistently, but from without, by rejecting its ultimate ends as too narrow. Hulliung implicitly rejects this possibility by maintaining that Rousseau always shared the Enlightenment goals of "freedom, individual autonomy and toleration"; but it is not so clear that Rousseau gave the same meaning and priority to these goals. In the end, it seems that the (preautocritical) philosophes were primarily concerned with externalities (security, negative freedom, property, toleration, and, above all, victory over their clerical enemies), whereas Rousseau alone refused to be diverted by these pressing external matters from the ultimate issues of God and happiness. On this telling, the Enlightenment always solicits its own critique because it fails to satisfy or address the most important human concerns.

If Hulliung's book fails to pursue as resolutely as one might like the inner core of the autocritique of Enlightenment, it is nevertheless indispensable for anyone wishing to do so. More generally, it restores to the study of Rousseau, so much of whose writings are a polemical response to the philosophes, the forgotten other side of the conversation.

Michigan State University AUTHUR M. MELZER

Locke in America: The Moral Philosophy of the Founding Era. By Jerome Huyler. Lawrence: University Press of Kansas, 1995. 394p. $40.00.

Locke in America is an ambitious work that takes on many significant historical and interpretative questions. It is more like two books of roughly equal length in one volume: one that tries to rescue Locke from the mistakes of various scholars and one defending the claim that most of American political thought from 1763 to 1800 was grounded on Locke's social and political principles.

The author thinks it necessary to write what amounts to two books because of a rather complex historiographical debate about Locke in America. The work of scholars such as Becker, Hartz, and many progressive historians led us to believe that Locke was crucial for an understanding American political thought, political institutions, and political practices. This view was challenged by a revisionist movement associated with the work of Wood, Bailyn, and Pocock, which sets classical republicanism in the place of Locke. The revisionist thesis, in turn, has been challenged by Appleby, Diggins, and Pangle, among others.

Huyler rejects the revisionist thesis conceiving of classical republicanism as a logical alternative to Lockean liberalism but acknowledges the importance of republicanism by adopting the revisionist theses in what might be called a weak form. Republicanism is compatible with fundamental Lockean moral principles. Hyler argues the methodological point that Lockean liberalism and classical republicanism were not separate and competing paradigmatic languages but, rather, different levels of discourse. Republicanism belongs to the science of politics, Lockean liberalism, to moral philosophy. The former, at least in America, was a means to ends set forth by the latter. Yet with a bit of rhetorical flourish, Huyler even claims that Locke himself was really an exemplary classical republican (p. 147).

While not accepting a republican revisionist thesis that pushes Locke to the sidelines, Hyler is still unwilling to embrace the work of any pre- or postrevisionist writer who emphasizes the centrality of Locke in America. Such scholars do not have the correct understanding of Locke—hence the need for an extensive examination of what Locke truly believed.

Huyler approaches the task of uncovering the fundamentals of Locke's moral philosophy by situating him within his historical context, a context of contending political and religious forces and ideas. He sees Locke as committed to natural right and natural law, to equal creation and equal protection, to reason and free inquiry, to scientific progress and industriousness, and to individualism. A brief summary list of his conclusions cannot do justice to the wealth of arguments, both historical and textual, that he uses to support them. Chapter 5, "A Benignant Egoism: John Locke's Social Ethic," is truly impressive. He masterfully rescues Locke from both Macpherson's aggressive egoism and Tully's social altruism. Throughout his discussion of Locke, Huyler takes on a variety of different scholarly interpretations and manages to score a great number of telling points. One does not have to agree with all of them to be impressed by his contribution.

Huyler claims that a lot of what has passed for "Lockeanism" would not have been thought so by Locke, yet he himself is not completely innocent in this regard. In the chapters on America, the historical John Locke almost drops out of sight—even at times when his actual thoughts might be of some interest. For example, in chapter 7, Huyler discusses William Penn (whom he oddly calls a "predecessor" of Locke on p. 41) and his 1682 *Frame of Government* to show how Lockean a conception of human nature it embodies. It would be of some interest to the reader to know that Penn gave Locke a copy of the text in Holland in 1686 and that we have Locke's very critical comments on it recorded in his journal.

The last chapter maintains that after the revolution and the founding, it was the Jeffersonians who expressed a deep commitment to Lockean principles in their opposition to Hamilton and his scheme for economic development and a national bank. According to Huyler, Hamiltonianism represented the *renunciation* of Lockean liberalism. It broke with Locke's liberal moral principles: "Equal privileges for all and special privileges for none; such is the essence of Locke's political ethic" (p. 306). Yet it could be argued that Hamilton was pursuing the common good in Lockean terms as he understood it and was not violating any fundamental postulates of Locke's moral philosophy. I doubt that the Locke, who was one of the original subscribers to the Bank of England and served out his days as a commissioner on the Board of Trade, would have seen service in Hamilton's Treasury department as a violation of his basic moral and political principles.

My most serious difficulty comes with Huyler's treatment of what he calls Locke's principles of "equal creation" and "equal protection." He gives them too egalitarian a twist. A socially egalitarian interpretation is not incompatible with Locke's principles and can be understood as an application or extension of Locke's principles. However, it is not logically entailed by these principles, nor was it the way Locke himself understood them. Locke would not have agreed with Americans as to what were and were not violations of his principles of equal protection and equal creation.

When it comes to the principles of equal creation and equal protection in America, republicanism often seems to move from the political science superstructure to the liberal moral philosophy substructure. In America, much of the attack on economic special privilege was a continuation of the attack on economic special privilege was a continuation of the attack on aristocracy. After all, in the *Second Treatise,* Locke assumes that a government comprised of a single hereditary executive and an assembly of hereditary nobility, along with a popularly chosen assembly, is perfectly legitimate. The British people returned to this form of government after a disastrous flirtation with a republic. This government did not conflict with the fundamental principles of equal creation and equal protection as understood by Locke, but it most assuredly would have conflicted with them as understood by the American founders.

Such doubts and disagreements notwithstanding, *Locke in America* is an important work of historical scholarship and interpretation. It makes a genuine contribution to the ongoing debate about Locke, liberalism, and the nature of the American founding.

University of Cincinnati DAVID RESNICK

Education, Democracy, and Public Knowledge. By Elizabeth A. Kelly. Boulder: Westview, 1995. 168p. $49.95.

Chaos theory might prove an apposite simile for educational policy and practice in the contemporary United States. Despite the work of an "education president," thousands of state-level initiatives, and the fanfare of Goals 2000, we currently have no coherent national approach to public secondary education. Coherence is not always a necessary condition for achievement, and the actual performance of our public schools is extremely hard to assess. Demographic changes, unreliable test data, and jeremiads notwithstanding, there have been achievements, as well as disappointments, at the micro and macro level in the academic performance of children. The basic statistics of reading, arithmetic, and science do, however, make somber reading; and the wave of "new ideas" for improving education continues unabated. Something, it seems, just must be done differently.

Academics have, in the last five years or so, joined in the debate. John Chubb and Terry Moe made voucher systems academically interesting. Allan Bloom did the same for relativism; E. D. Hirsch, for the *New York Times;* Amy Gutmann, for liberalism and education; Benjamin Barber for civic participation. Although the neo-Marxist and socialist perspectives have seen less publicity outside the academy, Stanley Aronowitz and Henry Giroux have kept alive the pedagogy of resistance. Elizabeth Kelly seeks to do the same for democratic socialism. Having recently published a book on democratic theory and education, I am keen to the challenges faced by Elizabeth Kelly as she joins the list. Since Kelly is not shy about the politics of her educational views, let me report that she seeks to make of Habermas' *ideal speech situation* an anchor point for "a *class* perspective on education" (p. 22). Such a perspective would demand that education provide "standards for public knowledge that critically confront the worst values and norms of capitalist democracy" (p. 61). The possession of such critical knowledge will also develop "autonomous, rational citizens," capable of "unconstrained forms of cultural and political communication" (pp. 75, 126).

Kelly's book is most "gracefully written," just as the dust jacket proclaims. Her effort to use Habermas as a starting point for a critical pedagogy is by no means misplaced, for Habermas has thought along such lines himself. If there is a divide between reformers who want to concentrate on market-based panaceas and those who, like Kelly, question the basic content of education, I confess to standing with Kelly. Moreover, in the process of clearing ground for herself, Kelly offers analytically forceful comments on a number of the scholars I have cited. For those who (like myself) are convinced that huge structural inequalities continue to be reproduced in our educational system, Kelly's angry rearticulation of that data comes as a familiar but not unwelcome gesture. That such reminders are out of academic and political season makes the conditions they report no less of a tragedy.

Trying to tie together political theory and pedagogic practice has been a perilous intellectual endeavor from pre-Socratic times. Does one collapse a political doctrine into a pedagogic practice so as transparently to politicize education? Alternatively, if one seeks a body of students intelligently critical of current ideologies (one's own included), how is a pedagogy to be arranged at all? Moreover, as political theorists, we cannot be confident that the ideas that make sense in our heads will not produce practical disaster. It is unsurprising that Benjamin Barber kept the positive suggestions in his book on education to one-tenth of its length. That Kelly falls well short of even that modest goal attests, therefore, to a common problem but also to more particular weaknesses. For example, while Habermas is clearly a key figure, less than a single page is spent analyzing the core of his thinking. The rest of Kelly's positive vision is something of an unsorted mixture light on substance. There are a few references to contemporary experiments, some lines about education as "a lifelong process of growth" (which could be directly quoted from John Dewey but for Kelly's earlier dismissal of Dewey), and some very Benjamin Barber–like sentiments about rebuilding community. Though her language is (untypically) ambiguous, I think Kelly adds to this list a demand for the abolition of private education (p. 123). While her sense of the consequences of inequality in educational practice might lead her to such a position, Kelly needed to support such a radical call against obvious and strong theoretical, as well as practical, objections.

A sophisticated pedagogic treatment of Habermas' thinking, building on the primary and secondary scholarship, might (and perhaps should have) formed a core to the boundaries Kelly lays out. In its absence, we find critical profiles of other thinkers and some good sense about existing proposals. What is frustrating about this book is that Kelly has skills, and they are not merely literary. Although the dismissal of Deweyian epistemology in one paragraph is unworthy of both her and him, there are perceptive remarks about other thinkers that simply never get developed. The tension between an ideological education (in this case, socialist) and an education critical of ideologies is never developed and is perhaps simply assumed away. Kelly talks of a way of "conceptualizing language as both immanent and transcendent" but then immediately switches away from this promising start to a largely unrelated set of observations (p. 102). The style of the book is a collage—here a reminder of inequality, there a sober remark about the limits of multiculturalism or a moving vignette of family history. For this reader—sensitive, I hope, to the different and moving voice in which Kelly writes—these and other parts of her work remain greater than their sum.

Vanderbilt University David M. Steiner

Hannah Arendt and Leo Strauss: German Émigrés and American Political Thought after World War II. Edited by Peter Graf Kielmansegg, Horst Mewes, and Elisabeth Glaser-Schmidt. Washington: German Historical Institute, Cambridge University Press, 1995. 208p. $49.95.

If I may generalize about diverse essays, this work addresses primarily the thought of Strauss (successfully) and Arendt (modestly) and contains little on American political thought. It chooses to focus on only two figures, leaving out others such as Morganthau, Voegelin, or Marcuse. The book consists of an introduction, three essays on Arendt, five essays on Strauss, and a concluding "discussion" among the participants at the 1991 conference, which addresses the three central issues: (1) the German intellectual background of Arendt and Strauss, (2) their intellectual evolution resulting from experiences in the United States, and (3) traces of their American experiences on their views of democracy. The major contribution of this book is to further our understanding of Strauss. It not only contains more essays on Strauss, but also, the essays on Arendt compete with a vast outpouring of articles and books on Arendt in the recent decade.

Dubiel reconstructs Arendt's theory of democracy as developed through the history of ideas and a comparison of the French and American revolutions. The essay by Kateb does not add much to his previous important contribution, *Hannah Arendt: Politics, Conscience, Evil* (1983). The Vollrath essay contends that Arendt's distinction between political and social spheres is taken from the German experience, noting (as have others) that this distinction was developed when Arendt confronted the totalitarian perversion and the inadequacy of political categories. In short, the essays on Arendt do not break much new ground; and in addition to journal articles, one should consult such recent studies as Gottsegen's *Political Thought of Hannah Arendt* (1994) and Canovan's *Hannah Arendt: A Reinterpretation of Her Political Thought* (1992).

Turning now to essays on Strauss, Fuller provides a perceptive analysis of Strauss's views on education and assesses American education, including examination of its relation to character and to democracy. Fuller believes the modern university has lost confidence in "the idea of learning for its own sake" (p. 67). He notes Strauss's emphasis on the great-books approach and identifies Strauss's argument as the idea that "modern democracy is principally not a regime but a mass culture managed by elites" (p. 70).

Gebhardt's excellent essay addresses the extent to which Strauss reflects the German tradition of mandarin intellectuals (1890–1933). Here, Gebhardt breaks away from Gunnell's myth-of-the-tradition interpretation and focuses squarely on Strauss's position that political theory is not merely an academic game, but must be understood as concerned with principles of social order. Gebhardt finds Strauss's notion of "cultural crisis" to be characteristic of the German intellectual community (mandarins) and convincingly argues that Strauss's "personal style and intellectual outlook reflect the habitudes and the intellectual vision of the German mandarins" (p. 88) with a concept of "self-cultivation" "based upon German classical culture" (p. 91). Gebhardt interprets Strauss on political philosophizing under modern conditions as needing our "ascent from the second unnatural cave of the modern worldview to the primary natural cave of the City" (p. 96). The scholar prepares the way for philosophy by accurate historical studies.

Mewes examines Strauss's relation to Heidegger and the meaning of modernity. Strauss increasingly sympathized with Heidegger's "revolution of thought" in that Strauss rejected "the pervasive belief in Western progress and enlightenment" (p. 105) and claimed the West was in crisis. Yet there was a profound *divergence*: Strauss returned to classical *political* philosophy, while Heidegger, in Strauss's view, did not allow for either ethics or political philosophy. Strauss criticized Heidegger's return to Greek antiquity as "under the sway of the most radical spirit of modernity," namely, "historicism" (p. 106). Strauss's resurrection of political philosophy becomes "a vindication of philosophy's discovery of nature as a genuine and lasting accomplishment" (p. 107). For Strauss, human nature, rather than Heidegger's "being," must serve as our touchstone.

In Söllner's view, Strauss's book on Hobbes focused more on Hobbes's moral basis than his new method, and Strauss was making an evaluative and critical comparison of cultural values. Later, Strauss became even more interested in the question of how far a thinker "deviated from classical natural right" (p. 131). Though Söllner provides useful description of Strauss's intellectual development, the Mewes essay is better on Strauss's relation to Heidegger.

The essay by Pippin addresses Strauss's wide-ranging "attack on the insufficiencies of modernity" (p. 139). Pippin focuses on the "wave hypothesis" and in particular argues that Strauss's interpretation of the second wave "misinterprets and undervalues the alternatives presented by, . . . especially Kant, Fichte, and Hegel" (p. 140). Pippin notes Strauss's view that there is a necessary hostility between political order (based on opinion) and philosophy, which, seeking wisdom, is inimical to the local conventions. Yet political life is judged by the standard of excellence—human perfection as represented by philosophy. Strauss's position is the anti-Enlightenment position that the world of human affairs is permanently in the cave. Pippin claims Strauss "neglects the larger issue of the *motivations* for the modern revolt against antiquity" (p. 147). While Strauss uses the idea of nature as virtually the central idea of his political philosophy, he "has little to say about a complex natural hierarchy, or chain of being" (p. 149, n. 30). Yet as Pippin rightly points out, it would seem to be necessary for a resurrection of the classical natural right position "that we be able in some way to identify the 'natural order of things', the situation of the human qua human" (p. 150). Pippin then examines Strauss's treatment of the alleged crisis in the thought of Rousseau and finds it inadequate. This essay is valuable for its full appreciation of the arguments of Strauss; the thesis regarding Strauss's overly hasty rejection of Kant is not as satisfying.

The discussion provides interesting comparisons between Arendt and Strauss on the influence of German philosophy and on their views on democracy. On Strauss, Deutsch and Nicgorski's *Leo Strauss* (1994) and Udoff's *Leo Strauss's Thought* (1991) are somewhat more comprehensive collections.

South Dakota State University GORDON J. TOLLE

A Civil Tongue: Justice, Dialogue, and the Politics of Pluralism. By Mark Kingwell. University Park: Pennsylvania State Press, 1995. 270p. $37.50 cloth, $17.95 paper.

A Civil Tongue is an intelligent and innovative contribution to the "dialogic turn" in contemporary theories of justice. Like others working in this area, Mark Kingwell sees dialogue and the mutual or civic understanding that can be engendered through dialogue as a procedure for mediating pluralism and arriving at just and legitimate solutions to political disputes. Also, like the majority of dialogue theorists, he assumes that if the conversation is to be productive and we are to continue to talk to each other despite our deep differences, then participation must be governed by a few basic rules. It is here, with the description and justification of the sorts of constraints that are appropriately placed on participants in public dialogue, that Kingwell makes his particular contribution to this debate.

Kingwell argues that public dialogue should be constrained by the idea of civility or politeness. He breaks civility down into two dispositions. The first is a disposition to restrain ourselves from saying all the things that we take to be true. So, to take a familiar example—and the familiarity of everyday civility is the whole point—the polite guest will not mention that her host overcooked the dinner. Here, the avoidance of hurt feelings or unpleasantness outweighs the need to disclose what one sees to be the truth. When this trivial example is translated into the public sphere, restraint would involve a willingness to forgo disclosing what one takes to be, say, a moral truth if such a disclosure would only serve to raise the hackles of one's interlocutor and cause dialogic impasse. The second disposition asks talkers to be listeners as well. This is called *interpretive sensitivity*, as it calls on interlocutors to rise above their own likes and dislikes and listen sensitively to what others have to say in an effort to understand their utterances. Cutting someone off in midsentence or turning one's back on a speaker is, under normal circumstances, usually thought rude. In the political arena a listening disposition amounts to a sense of impartiality and fairness.

The idea of restraint is very reminiscent of Ackerman's principle of neutrality and interpretive sensitivity resembles Habermas's principle of reciprocity. Thus, it is not surprising that Kingwell undertakes a lengthy discussion of these two thinkers in order to point out where civility differs from, and indeed improves upon, these two dialogic theories. What is surprising is that along with Ackerman and Habermas, there is a lengthy discussion of Alasdair MacIntyre. MacIntyre is read as a dialogue theorist defending "transtraditional" standards of rationality despite himself. Although this chapter does not mesh perfectly with the rest of the book, I found Kingwell's interpretation of MacIntyre to be one of the most interesting I have read. The chapters on Ackerman and Habermas are careful and fair evaluations of the strengths and weaknesses of their respective models of dialogue but they do not push debate forward in the way that the chapter on civility does.

Understanding constrained dialogue as an instance of civility is defended on three main grounds: an hermeneutical argument, a communicative competency argument, and a prudential argument. The hermeneutical argument is put forward mostly as a corrective to Ackerman. Particularly in *Social Justice and the Liberal State* (1980), Ackerman's neutral constraints appear from nowhere and are enforced by an extradialogical authority. But, says Kingwell, "unless the constraints on dialogue can proceed more organically from commitments you and I actually share, [Ackerman's] version of dialogue will not ultimately convince" (p. 94). In contrast, the idea of civility can be hermeneutical retrieved from our context: most of us understand and appreciate the benefits of at least a minimum of good manners and considerate behavior. Politeness oils the "wheels of mundane social interaction" (p. 211). Asking citizens to think about their public disputes as being governed by the rules of civility asks them to raise a set of familiar and everyday behavioral constraints into the political realm.

The communicative competence argument is put forward as a corrective to Habermas, who is accused of employing an idealized conception of dialogue. Appealing to sociolinguistic analyses of politeness, Kingwell argues that we should revise "what we consider rational, such that politeness becomes part of rationality after all; or it could involve recognition that rational is not all we want speech to be, even *or especially* when we want it to be normative" (p. 211). Kingwell's point is that Habermas is too concerned with *what* participants say rather than *how* they say it; but the latter is often just as important to the success of dialogue as the former, and anyone who is a competent speaker should know this. Whether or not this is a fair criticism of Habermas (which I doubt) is besides the point; what is important is that Kingwell does a masterful job arguing that politeness is not a superficial gloss on communication but essential to communication.

The final argument in defense of civility sets out to answer the question, "Why should I bother being polite?" In an attempt to avoid thorny philosophical issues about moral truth, Kingwell offers a pragmatic answer to this question very much indebted to Ackerman. We should cultivate good public manners in the "interest of social coexistence" (p. 42); politeness is "motivated by a desire to avoid debilitating conflict" (p. 69) because "manners reduce social friction" (p. 215). Civility is not defended on the grounds that we owe it (in some moral sense) to our fellow citizens. Instead, it is defended as the instrumental means to peaceful coexistence, something it is assumed that we all want. The problem with this argument is that it is not clear that we, in fact, need to be universally or indiscriminately civil in order to maintain social peace. Offending one's interlocutor will only result in debilitating conflict if one's interlocutor is in a position to disrupt the social order. This leads to the counterintuitive (at least from a moral point of view) conclusion that the more powerful (thus potentially disruptive) one's dialogue partner, the stronger the imperative to be civil. To avoid this problem, the obligation to treat *each and every* dialogue partner with civility must rest on a stronger moral argument than Kingwell is willing to offer.

University of Colorado, Boulder SIMONE CHAMBERS

Critique of Modernity. By Alain Touraine. Translated by David Macey. Cambridge, MA: Basil Blackwell, 1995. 398p. $54.95.

One of the peculiar attributes of the modern and contemporary understanding of modernity is that it is no longer contrasted with antiquity. It has come to be viewed as an independent autonomous topic. The great advantage of this self-understanding is that it permits elaborate and endless discourse that never quite becomes analysis. Instead, working within the unanalyzed assumptions of modern self-understanding, discourse (one may use impersonal terminology in such matters, though it is not compulsory) can construct its own linguistic entities and then turn around and—with equal rigor, enthusiasm, and skill—proceed to deconstruct. For reasons, perhaps, of quiet cultural pride in the face of military disaster, French intellectuals have adopted and adapted this German device to a degree greater than that of any other group of people. Alain Touraine is a master of the genre. Those who find the enterprise appealing will, on the occasion of the publication of this book, applaud the results as the work of a *maître-penseur*. Those who are puzzled by opacity of argumentation and bored by repetitive, allusive language bursting with metaphors may wish not to devote their time to his text. Taking a position in this way—no less than having the option of so doing—is a very modern way of proceeding.

Touraine takes as self-evidently true the position that "the idea of modernity was the assertion that men and women are what they do." Moreover, this understanding of human being, we are assured, is no more than three hundred years old. Furthermore it is "central" to our current ideas and practices and at the same time is being challenged, rejected, and redefined. These propositions, derived from the first two sentences of *Critique of Modernity*, are all highly questionable. The first, for example, which, following Schelling may be identified as a "meontological" assertion, was also present in an equivalent formulation in the Hebrew Bible or in the philosophy of Greek antiquity. There it was used as a symbolism to express the human response to divine presence in the soul. For the contemporary modern, however, the reality of the soul, to say nothing of a divine reality that might be present in it, has been so long eclipsed that it no longer exists even in memory. In the place of the soul Touraine has put Subject; in the place of divine reality he has put Reason. The substitution is indicated by the capital letters he uses when referring to these linguistic items.

Torn out of their more comprehensive experiential context that was indicated by the older and more analytically adequate terms of soul and divine presence, Subject and Reason can be connected, galvanized into motion, and identified with various names ransacked from the conventional and canonical history of philosophy. For example, Hobbes, Locke, and Rousseau are flagged as belonging (more or less) to one pole (Reason); and they are "critiqued" by (say) Nietzsche, Freud, and Foucault at the other pole. In this way, one proceeds from modernity triumphant to modernity in crisis. The result is an impressive tour de force in the most literal sense—but now what? True to the formal requirements of a history of modern opinion, Touraine ends by reciting his own curriculum vitae and concludes with a presentation of his most up-to-date and current position (pp. 368). He tells us of his earlier work on labor agitation and the distinction he drew between that agitation and the socialist conceits of progressivist intellectuals; later, he had argued that postindustrial society had replaced the production of material goods with the production of cultural goods; a third phase, based on his experiences of the events of 1968 in Paris, "defined" modernity in terms of "subjectivation," by which is meant "new forms of protest." That "appealed to the personality and to culture rather than self-interest." This brings us to his present position, which (again characteristically) is presented in the first person plural: "We no longer have faith in progress. We no longer believe that greater prosperity will lead to democratization and happiness . . . . We are increasingly afraid that growth will destroy basic natural equilibria, that it will lead to increased inequality on a world scale, and that it will involve everyone in an exhausting process of continuous change" (p. 371). Touraine adds other "worries that mask the underlying fear" that human being has broken with nature or with natural being and in consequence has become savage or barbarous.

The evidence of the phenomena is certainly persuasive enough, at least insofar as they express a sentiment of "alienation" (to use one of Touraine's terms) and a mood of solemnity. One would have thought that encountering this dead end would be an occasion to question the modern premises of modernity, to recall that modernity was in its genesis a rejection of something not modern. But as Touraine reports in his curriculum vitae, his formulation "is clearly historicist" (p. 368) and is, accordingly, unconcerned with "the general problems of the social order and democracy. Unlike political philosophy, it defines a society in terms of its work, its production and its capacity to act on itself." It is hard to improve on this formulation, save to add a minor postscript: Touraine has provided political philosophers with a symptom of the disorders they are called upon to analyze.

University of Calgary BARRY COOPER

Law in Civil Society. Richard Dien Winfield. Lawrence: University Press of Kansas, 1995. 204p. $29.95 cloth, $17.95 paper.

Law in Civil Society is a difficult book to read, but it may be well worth the effort. Richard Dien Winfield provides a modern restatement of Hegel's *Philosophy of Right,* though one with important modifications. Those unfamiliar with the assumptions or terminology of Hegel's work will, I suspect, find Winfield's book mostly impenetrable. He himself perhaps concedes as much when he writes that his project, which is to show "how reason attains normative validity by achieving a foundation-free autonomy," is so contrary to prevailing fashion that it may "have an almost unintelligible ring" (p. 30). But it would be a shame to ignore the book on account of its dense prose. Winfield raises extremely interesting questions; and for the persevering reader, the book's articulation of, and stimulus to think about, these questions is ample reward.

The questions Winfield takes up that seem to me most central and important are, What gives law its authority? and What role do the existence of political institutions and social practices play in determining the content of law?

In answering the first question, Winfield argues that law's authority does not derive from a constitution or political institutions that we consent to but from a conception of rights (similar to that which Hegel develops) that sees rights to our person and private property, rights to form a family, and free and equal access to a market economy as essential for our realizing our freedom. There is an idea of right prior to its being posited in law, although Winfield emphasizes that this idea is meaningless unless posited in law (chap. 4). Law ought to promote and protect these rights.

Winfield rejects foundationalist accounts that see the basis of law's authority as "extralegal" (p. 9). He is critical of those who ground law's authority in either consent or principles presupposed as valid, such as utility or other forms of "teleological ethics" (pp. 36, 160). He does insist that law be rational and not simply derivative of custom (pp. 92, 160). "An appeal to tradition or current convention," writes Winfield, "is of no avail because normative validity lies not in existence but in conformity with the determination of freedom" (p. 160, cf. p. 86). Winfield seeks a nonfoundational standard of rationality and finds it in Hegel. Winfield draws on what he calls "prescriptive logic," which looks very much like Hegelian logic, according to which we "must begin without any foundations and end up presenting the self-thinking of valid thought, which, as such, cannot help but be a wholly autonomous development" (p. 30). Using this logic, Winfield concludes that "justice is not a matter of embodying a highest good or being the outcome of the proper choice procedure or cohering with any historically given community. Valid practice instead consists in nothing but the concordant exercise of rights in all their different spheres" (p. 32, cf. p. 9).

I doubt whether such language, which pervades the first two chapters, will be persuasive to those not steeped in Hegel. These chapters claim to explain the *necessity* of recognizing certain rights and claim to do so without presupposing the existence of social practices. One weakness of the book is, I think, that it does not sufficiently justify this claim. Hegel himself can be understood to see these rights not as "natural" (in the sense of existing prior to certain conventions or norms) but as derivative of these practices and institutions. One way Hegel supports the claim that these practices and institutions are rational and the proper source of a conception of rights is that they developed in history, which is purposive and rational: Hegelian metaphysics underpins this claim. Winfield does not say whether he appeals to a similar metaphysics, nor does he otherwise explain sufficiently the *necessity* of private property, family (he denies it is a biological necessity), or a market economy. He says, for example, that "stripped of the right of ownership, an individual is no longer a person with a respected objective domain of self-determination" (p. 36). But if this is true, it seems true only for particular propertyless individuals within a propertied society. But why can't we imagine a society where possession of property is not essential to personality? What is the *necessity* Winfield alludes to in saying that ethics "must" begin with property rights (p. 35)?

One of the most interesting claims Winfield makes—a claim that accounts for the title of his work—is that law properly understood exists in civil society and can be conceived "prior to the conception of the state" (p. 89). It is universal, applicable not just to human beings but to any intelligent beings who participate in civil relations: twice, Winfield suggests that law properly understood might apply to extraterrestial life if it ever were found (p. 186, n. 41 and p. 186, n. 3). This universalism is connected to Winfield's anticonventionalism, his denial that customs and practices are the source of the content of valid law. Winfield rejects appeals to background norms in specifying law at a general level (see p. 42), although, following Hegel, he concedes the need for turning to custom and positive law to specify the details of law, such as how much punishment a particular crime deserves. The denial of the importance of social practice in shaping law and the idea that law can be universal in this way, seems to me wrong; but Winfield gives a useful statement of the position. That Winfield takes Hegel's *Philosophy of Right* to have this universalist implication and to entail philosophical anarchism—the view that one's political identity plays no role in one's obligation to obey law ("When law in civil society operates within a particular state, legal authority extends to citizens and noncitizens alike," p. 98)—points to another interesting aspect of the book that marks it as a significant contribution to scholarly debate: *Law in Civil Society* can be read as a particular interpretation of Hegel that emphasizes Hegel's cosmopolitanism. This would be a controversial reading, however, given that Hegel is usually read as a communitarian, explicitly criticizes Kant's cosmopolitanism, and emphasizes the priority of social practice to principles of right. (As noted earlier, Winfield does not always mean to follow Hegel. He criticizes Hegel occasionally—for example, for commending rehabilitation as an aim of legal punishment [p. 187, nn. 14, 15], for defending jury trials [p. 191, n. 41 and 146–50], and for privileging estate membership [p. 148].)

One important implication of Winfield's view that legal rights in civil society are logically prior to a determination of constitutional rights is that they may provide a standard for criticizing or guiding constitution making. In addressing whether legal rights are best protected by an adversary or inquisitorial system, Winfield writes that "whether legal right in civil society mandates adversary practice is what dictates whether particular constitutions warrant revision" (p. 160). Winfield draws on his Hegel-inspired account of law to make several prescriptions. He argues that law and legal advice should be accessible to all and provided by the state (p. 121), though he does not consider how this would be implemented in light of the considerable power of those with an interest in maintaining the current practice of lawyering. He argues that family law should not be based on sexual preference (p. 60); he rejects both the right of the accused against self-incrimination and the right of confidentiality between lawyer and client (pp. 135–38); he criticizes undue prosecutorial discretion, argues for a right to legal counsel in civil cases and public intercession in civil lawsuits, and opposes the exclusionary rule (pp. 153–55, 194, n. 108)—in all cases supporting his position with his normative account of law in civil society. His criticisms and prescriptions appeal not to empirical evidence from social scientists but to what has a "normative edge" (pp. 163, 166). These policy recommendations, while controversial, help show what is at stake for Winfield in developing a Hegelian jurisprudence. By conceiving law as for the protection of individual autonomy and freedom, we can shape that law so that it better promotes this purpose. Even those who won't agree with Winfield's Hegelian account of law can learn from his ambitious and inspired example of legal and social criticism.

Stanford University MARK TUNICK

Natural Rights and the New Republicanism. By Michael P. Zuckert. Princeton: Princeton University Press, 1994. 397p. $39.00.

The point of departure for this painstaking and exhaustive account of the origin and development of Whig political philosophy is a comparison of the English Declaration of Rights and the American Declaration of Independence. The comparison indicates that there is only, at best, a slight theoretical connection between these two great political documents. While the Declaration of Rights derives its dignity from positive laws and customs—the "ancient rights and liberties" of England—the Declaration of Independence relies on the "laws of nature and nature's God." As Zuckert notes, the contrast could not be more compelling: "The place of rights in the Declaration of Rights shows it to have been an important document in the rise of constitutional government, but it does not express the same understanding of the nature and purposes of government as the Declaration of Independence does. It could not produce, as the natural rights philosophy was to do, the new republicanism" (p. 13). The radical core of the "new republicanism," it seems, is the substitution of nature for history or prescription as the ground not only of rights but also of obligations.

Obviously the thought that produced the Declaration of Rights could not have been the same that produced the Declaration of Independence. Zuckert successfully argues that contrary to conventional scholarship, Whig political philosophy is not a single, unified whole dominated by Locke. Indeed, it seems that the seventeenth-century Whigs were, generally speaking, followers of Grotius, whereas Locke comes to prominence only the eighteenth century. It is Locke—transmitted largely through the popular letters of Cato—who provides the radical point of departure for the American Revolution and the American founding.

Zuckert well understands the dimension of political philosophy that is concerned with the "theological-political problem." Whig political philosophy was, by and large, an attempt to make its peace, so to speak, with the Reformation. The vehicle chosen for this attempt at accommodation was a radically transformed conception of natural law. Natural right had been forced to speak the language of natural law after the advent of Christianity. A universally valid law (rather than a dynamis that is everywhere changing and dependent upon practical wisdom for its implementation) became the goal of natural law thinkers. While there was still a strong element of Aristotle in Christian natural law thinkers, the natural law that formed the horizon of Whig political philosophy was of an entirely different order.

According to Zuckert, Grotius was *the* Whig philosopher of the seventeenth century. But Grotius appears to be a transition figure, attempting to bridge the rapidly disappearing world of thought dominated by Aristotle (or the Aristotelian Christians) and a new world dominated by "a theologically neutral natural law" (p. 149). But it is Locke who dominates this study; and it is in the interpretation of Locke that Zuckert excels. In Zuckert's interpretation, Locke provides a radical departure from both the Christian Aristotelians and Grotius: "Morality is not the first principle of human action, as the Thomists and Grotius had it. The ground of 'natural law', or morality, is now understood by Locke as natural right, and therefore the main requirements of morality can be restated in terms of rights" (p. 214). Here, of course, Locke (following Hobbes) understood natural right simply as the right to self-preservation.

Locke uses the language of natural law out of a regard for political-theological expediency. But the core of Locke's account is the primacy of natural rights, not natural law: "Natural law, far from being primary and the source of rights, is secondary and derivative from rights" (p. 273). The natural right to property—derivative from "self-ownership" rather than "divine authorship"—creates natural obligations. As Zuckert notes, "To say there is property by nature is to say there is justice by nature." Thus, Locke seems to differ from Hobbes, who "strongly insists that there is no natural justice" but only justice based on positive law and derivative from contract. In Locke, if Zuckert is correct, there is thus a natural ground for inalienable rights because "they inhere in the very structure of the self" (p. 285). The self is itself property, a "self-acquisition" as it were; "The self is just that empty center of consciousness that contains and possesses all its appearances. The self as empty center of consciousness—pure ego—is owner; the self as array of possible ideas and actions is owned" (p. 285). Most important, however, is the fact that the natural right to property precedes civil society and thereby provides the ground for natural obligations. The right to property necessitates the duty to forbear on the part of others, which "affirms a preexisting normative right, which, ineffective as it may be in itself, grounds the entire edifice of political life and political philosophy." Thus, "human making, not divine making, is the primary moral fact. . . . Human beings are self-owners because they are the makers of their selves and they own what they make" (p. 278). Zuckert concludes that "the claim to property in one's own person is thus simply central to Locke's enterprise. . . . It points toward his break with the entire premodern tradition" (p. 276).

One interesting question only briefly touched upon by Zuckert is how Locke was understood by the American founders. Did they understand him as a radical modern asserting the self-acquired ego as the ground of natural rights? Or did they see only the exoteric Locke who argued that natural rights were derivative from the laws of nature (and nature's God) understood as reason and confirmed by revelation? The exoteric Locke points, however tentatively, in the direction of Aristotle. Zuckert, however, seems to think that the question is not so important, because "Locke deploys quasi-traditional forms of argument to lead readers to conclusions mostly congruent with and ultimately derivative from his wholly untraditional premises" (p. 288). It is certain that the framers did not read Locke as an esoteric version of Hobbes; nor did they read him in the light of the radical distinction between ancients and moderns, as Zuckert does. In any case, the framers were not simple Lockeans; they were also statesmen and their understanding of political prudence was surely not derived from Locke's radical egotism. But these are questions that Zuckert will undoubtedly take up in another study. In the meantime, Zuckert has produced an account that is far superior to anything in the work of J. G. A. Pocock or Gordon Wood.

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EDWARD J. ERLER

AMERICAN POLITICS

Expecting Trouble: Surrogacy, Fetal Abuse and New Reproductive Technologies. Edited by Patricia Boling. Boulder: Westview, 1995, 201p. $55.00 cloth, $18.95 paper.

The maternal-fetal relationship continues to be reshaped by the changing technological context of prenatal medicine and by our expanding knowledge of fetal development. Moreover, a legal system based on adversarial relationships and assignment of proximate cause has heightened attention on the actions or omissions of pregnant women that might be harmful for the fetus. The proliferation of new prenatal diagnostic and therapeutic techniques, new reproductive technologies and their social extensions (e.g., surrogacy contracts), and the continual medicalization of all aspects of pregnancy, along with the courts' involvement in these issues contribute to the creation of situations where the rights of the pregnant woman are seen as conflicting with the perceived interests of the fetus (or the child once born) to a sound mind and body. These technological, social, and legal trends raise serious concerns over their implications for women's rights in the 1990s.

This collection of essays, which grew out of a 1992 APSA panel on government control of reproduction, addresses these issues in a way that should be of interest to feminist theorists and students of American public policy. Given the large volume of work in this area published in the last several years, I cannot judge whether it represents an "important advance in feminist political theory" as suggested by Nancy Hartsock in the foreword (p. vii). However, it does represent a valuable synthesis of feminist theory and public policy. Most of the chapters are clearly written and extensively annotated. The lists of references at the end of each chapter are very useful and collectively might serve as an authoritative review of the current literature. Although many chapters are interesting and raise thought-provoking policy issues, few could be considered groundbreaking.

The subtitle of the book is misleading to the extent that only two chapters deal with surrogacy and one with reproductive technologies. The remaining chapters focus on issues of fetal harm and the resulting challenges to the rights of pregnant women. The context, therefore, is skewed to the latter topic at the expense of balanced coverage of the broader range suggested in the title. This volume compares favorably to similar anthologies and might serve as a useful introduction for those readers who are not familiar with the relatively sophisticated literature in this area (see, e.g., *Women and Politics,* vol. 13 [1993]). Four of the nine substantive chapters in *Expecting Trouble* are reprints of recent journal articles. The editor, however has done an excellent job in selecting appropriate reprints to supplement the original contributions and has provided a useful introduction to the themes and issues of the volume.

Patricia Richards's chapter on tailor-made children is a good introduction to the challenges to women raised by new reproductive technologies. She demonstrates that while these techniques are often presented as expanding women's rights, commodification of reproduction and increased social control raise serious questions about the adequacy of the rights' framework itself. This results, in large part, because while the liberal concepts of rights and choice "presume no serious difference in power among social groups" (p. 17), reality proves otherwise. Unfortunately, this chapter was not accompanied by other chapters that could carry on a more in-depth analysis of the critical issues surrounding the impact of reproductive technologies on women.

The chapter by Stickland and Whicker is a valuable descriptive contribution on the discretion given prosecutors in determining criminal liability of pregnant women accused of endangering the fetuses they carry. After discussing the major arguments for and against criminal prosecution, this original chapter describes the findings of a survey of state attorneys general regarding fetal abuse. They find that although no state has a statute that explicitly makes it a crime for a pregnant woman to expose a fetus to substance abuse, local prosecutors have used a number of existing child welfare statutes to charge pregnant women. They conclude by arguing that this discretion given local prosecutors is counterproductive and may serve only to drive pregnant addicts underground in fear of losing their parental rights, "ultimately making the cure worse than the disease" (p. 75).

The Schroedel and Peretz chapter makes a valuable contribution by showing how cultural predispositions and resulting gender biases produce a situation where women are blamed for fetal harm while the role of men, especially evidenced through spousal abuse, is ignored. They also note that feminists, by and large, were caught off guard by the framing of fetal abuse as a problem caused by lifestyle choices of women. In her chapter, Lisa Bower effectively weaves in the race factor by demonstrating how the "fetal harm debate is sustained by a tropological dependence on race" (p. 142). Too often, the targets of the legal process are single, black women; but more important is how the "legal fiction of individual choice suppresses the articulation of race in accounts of the prosecution of substance-abusing mothers" (p. 144).

Phillip Johnson's short chapter is an interesting inclusion to this volume and provides balance to other chapters. He argues that while the law cannot compel virtue, the American Civil Liberties Union philosophy that pregnant women have a right to use drugs without fear of prosecution is misguided. There is no right to abuse children, born or unborn. While criminal prosecution is not ordinarily an effective way to deal with fetal abuse, it may serve the "purpose of demonstrating that the community has certain norms of behavior and takes them seriously" (p. 138).

The issues discussed in *Expecting Trouble* will continue to intensify as increasing attention is directed toward fetal health. Given the high stakes for pregnant women, it is critical that feminist thinkers play an active role in forging a reasonable and workable framework for resolving these dilemmas. The social construction of fetal rights by the courts and by society, largely in response to new technologies, medical knowledge, and the medicalization of pregnancy, raises questions concerning the viability of the rights' framework. Moreover, confusion over the distinction between any moral obligations a pregnant woman might have to the fetus she carries and the imposition by society of a legal duty to behave in a particular manner requires a considerably expanded dialogue. Although this book does not provide any definitive guidance to that end, it offers many valuable insights that should be flushed out in future work of these authors.

University of Canterbury ROBERT H. BLANK

Politics and the Class Divide: Working People and the Middle Class Left. By David Croteau. Philadelphia: Temple University Press, 1995. 285p. $49.95 cloth, $17.95 paper.

David Croteau's *Politics and the Class Divide* is a fine attempt to explain the middle-class base of left and left-liberal social movements. Croteau explores the reasons behind white working-class nonparticipation in movements that are working for fundamental social change in the United States. Contrary to public images of the white working class, Croteau argues that the white working class is not conservative but actually shares many of the views of left political activists about political change in this country. Although the white working class and left political activists recognize similar problems, middle-class activists and white working-class people have come to very different conclusions about what is possible to do about the problems. For Croteau, the different conclusions are reached by very different class experiences and worldviews.

Croteau uses a rather simplistic distinction of class, the mental/manual division. Yet, for his purpose of explaining different class cultures, this rather loose definition of class seems the right way to go. By avoiding the rather abstract notion of structural class divisions and boundaries, the loose way that Croteau defines class allows him to ground his analysis in real people's experiences and worldviews. This is the real beauty of the book. Although his sample was small (44 individual workers employed in a nonunion printing plant and mailing house in a small New England city), he is adequately able to capture working-class people's lived experiences and their views of political life.

Croteau develops his argument by first discussing, both theoretically and historically, evolving notions of democratic participation and the rise of both left movement and "new social movements." Croteau, moreover, develops a conceptual framework for understanding class and culture that lends itself nicely to empirical research—a rarity in today's academic marketplace. The first part of the book establishes the framework for posing the question of nonparticipation of the white working class in progressive social movements that seeks to locate the source of explanation in the political system and left movements, as opposed to a unique problem of the American working class.

Croteau argues that the lack of a broader social vision of the labor movement in the post–World War II years, especially its lack of involvement in civil rights and the purging of leftists at the end of World War II, left a void that was eventually filled by new social movements concerned with issues of peace, the environment, and broader social issues. The distinctly middle-class nature of these movements has, by and large, prevented working-class participation.

Through a series of interviews with both middle-class activists and working-class nonparticipants, Croteau found that what divides the two classes from working together is basically their understanding of the political world, which is rooted in their daily living experiences. Middle-class forms of knowledge and communication enable middle-class activists to work within the contemporary political system, whereas working-class people bring little resources to participate in a political system that is often perceived as unfair and closed to any meaningful participation to affect change in the broader social landscape.

Croteau concludes that if middle-class movements are going to be effective in evoking broad social change, they need to diversify their class base. The way to diversify their class base is to provide a "politics that affirms basic values of justice and fairness in practice and pursues constructive programs for change that enhance the quality of daily life at work, at home, and in the community" (p. 219). In short, for Croteau, progressive democratic politics can only succeed if middle-class activists reach out to the real, lived experiences of the working class.

While *Politics and the Class Divide* is an excellent book that is accessible for undergraduate students and provides a solid argument to present to students in a variety of courses (e.g., political sociology, social movements, political participation, political participation), its insight into the complexities of working-class life in the United States is a bit limited. The changing racial composition of the American working class and the stratification within the class itself is probably a bigger class divide than that between the white stable working class and the middle class in contemporary America. It just may be the case, if we were to follow the logic of Croteau's argument, that the issues that new social movements are concerned with are not the most pertinent to the daily lives of the majority of the American working class. But this is a topic for another book.

Colgate University RHONDA F. LEVINE

Presidential Lightning Rods: The Politics of Blame Avoidance. By Richard J. Ellis. Lawrence: University Press of Kansas, 1994. 262p. $29.95.

This book resurrects, and builds on, Pendleton Herring's more-than-50-year-old argument about the limits of presidential responsibility. Herring believed that presidents ought not to carry the full burden of accumulated grievances that follow from minor defeats and that effective presidential leadership depended, in part, on making lesser officials accountable for particular policies in order to protect the capacity and authority of the president. Richard Ellis firmly adheres to this view and uses it as the normative base for an empirical study of the conditions under which a president can deflect blame onto subordinates, or "lightning rods" (as he labels them).

Ellis's search for presidential lightning rods is painstaking, conscientious, thoughtful, and well crafted. He seeks them from among vice presidents, White House chiefs-of-staff, secretaries of state, other cabinet members, and even presidential spouses; and his search constitutes an interesting and original exploration of a phenomenon that has hitherto been taken for granted, rather than studied systematically. For this reason, *Presidential Lightning Rods* is a welcome contribution to the literature on the presidency. It points the way to further and necessary empirical research on the politics of blame avoidance and may possibly reopen a neglected but now very relevant debate about presidential accountability.

Unfortunately, Ellis's search for lightning rods yields few really solid examples. Of all the cases he analyses, only two—Ezra Taft Benson and Vice President Richard Nixon—fully conform to the author's strict definition of a lightning rod, that is, a presidential subordinate who not merely attracts criticism but deflects it away from the president.

The shortage of true lightning rods does, however, give Ellis plenty of scope for drawing out just how tenuous, uncertain, and difficult it is for presidents to pursue a lightning-rod strategy; and he does this well. He shows that the scope for lightning-rod strategies is severely constrained by numerous factors, not least that any president "who is serious about blame avoidance must be prepared to cede not only responsibility but power" (p. 144). Ellis also argues persuasively that some subordinate positions, particularly secretary of state and White House chief-of-staff, are, by their very nature, unsuitable for the lightning-rod strategy. Furthermore, the changed nature of the vice presidency over the last 20 years is now likely to make the occupant of that office less useful as a presidential scapegoat.

The success of lightning-rod strategies will also depend, Ellis asserts, on how mass opinion responds to elite discourse. This is, perhaps, the most tenuous aspect of all because political elites are not fooled by the lightning-rod ruse, and the linkage between elite and mass opinion is a "more uncertain and uncharted ground" (pp. 110, 154).

In the end, there is little Ellis can say with certainty about the conditions under which a president can deflect blame onto subordinates, and he is quite honest about this. "We have unearthed no precise formulas for identifying let alone predicting the existence of lightning rods," he writes. "The cases are too few, the variables too numerous. . . . Generalizations about lightning rods must be constructed upon an inhospitable terrain in which exceptions leap readily to mind and contingency and idiosyncrasy abound" (p. 145). He even recognizes that some people might suggest that time spent in this endeavor is wasted, but it is not. At the very least, Ellis's work ought to make others wary of using the lightning-rod metaphor when discussing blame avoidance in the presidency.

Notwithstanding these constraints and uncertainties, Ellis believes it is still possible, indeed desirable, for presidents to pursue a blame avoidance strategy using subordinates as lightning rods. The persuasiveness of his case rests, in part, on his claim that presidents are presented with a basic choice in leadership style between blame avoidance and credit claiming, a dichotomy that is at least questionable and may well be a serious oversimplification. Nevertheless, because Ellis sees presidential leadership style and temperament as a prime determinant of successful blame avoidance, he holds up Eisenhower as the model for "keeping out of trouble" and the crucial question is whether Eisenhower's blame-avoidance strategy is, as Ellis claims "just as relevant today as it was in the 1950's" (pp. 172, 183).

Eisenhower revisionists will be sympathetic to this argument (Ellis's book is also an important addition to the Eisenhower revisionist literature), but others will be more skeptical. They will see more in the transformation of American politics since the 1950s than Ellis does. He thinks those changes "are often greatly exaggerated" (p. 172). They would also focus on specific changes that Ellis understates. For example, the role of the media is at issue here; and although Ellis mentions the rise of an adversarial media since the Eisenhower years, he neither dwells on this point nor seems to recognize the extent to which the preoccupation of White House correspondents with the strategic calculations behind presidential behavior make it far less likely that any contemporary president would be able to succeed with a deliberate, hidden-hand, blame-avoidance strategy of the type that marked Eisenhower's leadership style.

But more important than the empirical problems with Ellis's normative vision is the vision itself. His conclusion—that placing a premium on staying out of trouble may now be a prerequisite for effective presidential leadership (p.183)—when combined with a prescription based on the Eisenhower hidden-hand approach characterized by strategic ambiguity, is not likely to persuade those who believe there is already too much ambiguity associated with presidential behavior. It will, however, engage those concerned with differing notions of presidential accountability and ought to reawaken interest in the kind of debate that Pendleton Herring generated in the 1940s but that has not been high on the agenda of presidential scholars in recent years.

There is more to be said about blame avoidance and the presidency. Given the difficulty of pursuing lightning-rod strategies, Ellis's work ought to stimulate the study of other blame-avoidance strategies. It might also generate discussion of some of the assumptions inherent in the blame-avoidance approach. Truman, for example, did quite well with a blame-acceptance strategy, symbolized by his Buck Stops Here sign, and John F. Kennedy reached the height of his popularity when he accepted responsibility for the Bay of Pigs fiasco. Finally, this book may raise questions about the distinction, sometimes blurred by Ellis, between presidents who keep out of trouble and those who deflect blame for the trouble they get into.

Richard Ellis has produced a stimulating and challenging analysis of an important aspect of presidential leadership. Not all will agree with his conclusion, but few can deny that his work opens up an area of empirical and normative significance.

Australian National University JOHN HART

The Public City: The Political Construction of Urban Life in San Francisco, 1850–1900. By Philip J. Ethington. Cambridge: Cambridge University Press, 1994. 464p. $54.95.

At the March 1995 meeting of the Organization of American Historians, more than two hundred historians of the United States gathered to discuss the revival of political history. The advent of social history, labor history, women's history, black history, hispanic history, and other histories have made political history unfashionable and little pursued. One reason political history faded was that its traditional subjects—powerful

Anglo men, now out of fashion—so dominated the genre. How, one participant asked, could a revived political history possibly embrace all the new histories yet present a coherent story? More important, what should the agenda of a revived political history be?

In *The Public City*, historian Philip Ethington successfully takes these tasks as his own. Ethington's agenda is largely the story of parties, elections, and associated social movements. This potentially traditional tale is newly told here. Ethington presents a unified political narrative in which the subjects of social histories are not mere afterthoughts to the achievements of their social betters. The Women's movement provides a good example. Three generations of advocates of women's suffrage make their appearances here. They failed to persuade the men of San Francisco to support them; even when women's suffrage was adopted in California, it was without the support of San Francisco voters. Ethington shows that the battle over women's suffrage was simultaneously a battle over the proper functions of local government. (Mayor Phelan himself opposed both women suffrage and the agenda for municipal government that women advocated.)

Ethington is insistent on the determinant role of politics. Rather than reduce politics to social changes, Ethington urges historians to be attentive to the public and political causes of political events. For example, Ethington dismisses the usual story of late-nineteenth-century parties deeply entrenched in neighborhood attachments and benefitting from voters' unquestioning party loyalty. San Francisco voters were actually quite fickle in their choices. The unassailable edifice of party power was built on control of the casting of ballots. Voters were required to force their way through crowds of party workers distributing ballots printed by the parties; and once inside to cast their ballots, they were still not safe, making voting altogether a rough and often violent ordeal (pp. 74–77). Disaffected politicians and labor organizers destroyed party control of elections by successfully campaigning for the Australian ballot. "Not changes in society or the economy," the author concludes, "or some groundswell of public opinion rising from the 'base' of the polity, but specific political innovation at the top of the political structures explains the advent of progressive reform in San Francisco" (p. 344).

Ethington also argues that urban political history should center on what he terms "the public sphere." The public sphere is the "realm of communication" linking "state" and "society"; its central institution is the press. It is in the public sphere that political culture is articulated, naming collective identities and promoting selected values. Alongside the narrative of elections, the Vigilante movement, the reverberations of civil war mobilization, the Workingmen's party, and progressive legislation is the narrative of the public sphere. In 1850, public discourse still centered on the eighteenth-century oppositions of virtue and corruption, independence and slavery. By 1900, political discourse exhibited a shift to "pluralist liberalism," in which social groups competed for public favor and influence (an understanding of political life soon to be rigorously articulated by Arthur F. Bentley). This latter public discourse was the setting and precondition for progressive era politics.

Ethington's account of the public sphere is not altogether successful. On one hand, Ethington is to be admired for joining the academic conversation about rhetoric and discourse, arguing the importance of a public "script" in political life. On the other hand, although Ethington claims a critical role for the discourse of the public sphere, one is hard-pressed to find in his narrative persuasive examples of the importance of language. For example, even conceding the increased importance of the popular press toward the end of the century, it is unsettling to consider a framework in which William Randolph Hearst is as responsible for the beginnings of progressive politics as, say, San Francisco Mayor James Phelan or California governor Hiram Walker (although one imagines Phelan and Walker were themselves probably unsettled by Hearst).

This is an impressive book. The arguments are closely bound to the narrative, the research is monumental, the theoretical stance fertile. Like all long and ambitious books, this one offers many opportunities to quibble. I suggest that readers forgo those opportunities and instead savor this well-presented food for thought.

University of California, San Diego AMY BRIDGES

The Fiscal Crisis of the States: Lessons for the Future. Edited by Steven D. Gold. Washington: Georgetown University Press, 1995. 406p. $42.50 cloth, $19.95 paper.

Now that the libertarian wing of the Republican party controls the national political agenda, it is a good time to read a book about the states. This is because the easiest way to reduce the size of the federal government is to shift unwanted programs to the next-lower tier of government. Of course, the states are said to be "laboratories" for policy innovation, bureaucratic flexibility, democratic responsiveness, fiscal responsibility, and institutional efficiency. Yet *The Fiscal Crisis of the States* shows that in response to a fiscal crisis, the states are just as likely to pass their obligations to others, cut services for the politically weak, and use accounting gimmicks balance their budgets. Not all states responded this way to the 1990–91 recession; but enough did to raise doubts about the utility of using block grants to transfer responsibility of programs to the states, especially if the grants imply a reduction in real funding levels.

The stated purpose of the edited volume is to give a detailed description of exactly how the states responded to their latest fiscal crisis. A fiscal crisis is defined by editor Steven Gold as a "significant imbalance between revenues and expenditures." And if it seems that a fiscal crisis is chronic among the states, it is because their tax systems are "dysfunctional"—meaning they have become unresponsive to economic change. The states are either too reliant on property tax or have been slow to tax services. Even a mild recession causes major distress among state budget managers.

The volume is actually two books in one. The first presents an overview of state fiscal conditions in all 50 states. The second is a collection of case studies of 6 states: California, Connecticut, Florida, Massachusetts, Michigan, and Minnesota. Busy scholars and public officials looking for a brief account can limit their reading to the concisely written overview and comparative chapters. Those wanting a more in-depth story can delve into the tangled world of budget politics by reading the case studies. It is clear that Gold took resolute editorial control, as the state chapters follow a parallel outline.

Each study begins with an overview of the case followed by a detailed, year-by-year description of the causes of the crisis, the revenue and spending strategies used to overcome the crisis, detail about new initiatives, and a summary clarifying the budget winners and losers. The common outline may have constrained the creativity of the various authors, but it greatly eases comparison between cases.

While one can get a synopsis the 50 states, the "real story" is in the details. The book dispels the commonly held notion that unfunded federal mandates led to the states' fiscal crisis. Federal funding increased significantly and helped the states narrow their deficits. In particular, provisions in Medicaid reimbursements allowed states to shift costs to the federal government by transferring formerly state-funded programs to Medicaid or by increasing payments to providers to qualify for federal matching funds with the understanding that the money would be taxed so the state could recoup, then reallocate the money. The Health Care Financing Administration estimates that the provider donations and taxes cost the federal treasury $11 billion in 1992 alone. Michigan took the greatest advantage of this loophole to garner an additional $200–250 million annually between 1990 and 1994. Massachusetts governor Weld rewarded the state employee who found the loophole with a $10,000 bonus as part of his "reinventing government" program.

The case studies also show that states can avoid tax increases or spending cuts by using accounting gimmicks to defer costs to the future. For example, California arbitrarily assumed that the state pension fund would increase its yields so they could reduce the state's contribution. Massachusetts consistently counted revenues from the sale of state assets that went unsold. In addition to overcharging the federal government for Medicaid expenses, Michigan avoided tax increases by delaying payments to school districts and local governments. Still, the states cut expenditures and increased taxes. In general, the losers were welfare recipients and local governments. Several states cut K–12 education programs, and most cut higher education and increased tuition fees. All the states (except Michigan) increased taxes or user fees. Connecticut completely revised its tax system, moving from a reliance on sales to income tax. Florida, took a less courageous strategy by increasing "sin taxes," levying new taxes on newcomers and tourists, and raising user fees to the extent that the president of the state Senate characterized the increases as "Excuse me for living" taxes. Surprisingly, voters did not seem to punish the tax increasers.

Thus, the book made good on its promises. It looked beyond the public relations rhetoric and told how state government actually responded to the challenges of the early 1990s. One of my complaints is that the short (four-year) time frame was normally too limited to tell the complete story. In part, this limitation was made up by individual authors who started their stories one or two administrations prior to the crisis. For example, Minnesota completely avoided a fiscal crisis despite the recession, by restructuring their tax system in the early 1980s and capitalizing a healthy rainy-day fund. My other complaint about the book is its lack of a theoretical framework from which to view the crisis. Several new theories of federalism have surfaced in recent years that move beyond the static notions of "layer cake," or cooperative federalism. Most notable are Erickson, Wright, and McIver's notions of statehouse democracy and Paul Peterson's dynamic public choice analysis. Finally, although not a main thesis of the book, I remain skeptical that a fiscal crisis is a sufficient condition to spur to positive innovation. As the Minnesota and (to a lesser extent) the Massachusetts cases show, some fiscal slack is needed to create an environment of flexibility and openness to innovation.

University of Notre Dame MARTIN SAIZ

Challengers, Competition, and Reelection: Comparing Senate and House Elections. By Jonathan S. Krasno. New Haven: Yale University Press, 1995. 195p. $26.00.

Although Senate elections have been more competitive than House races for over two decades, scholars are still uncertain about the underlying causes of this phenomenon. Some students of congressional elections have argued that senators are more vulnerable because of their visibility and diverse constituencies, while others contend that senators confront more effective challengers. Jonathan Krasno's comparison of voter perceptions of House and Senate candidates drawn from the 1988–90 Senate Election Survey (SES), supports the latter view.

Krasno aims at settling old questions in the literature, rather than offering new interpretations, but his study is significant because it is the first book-length examination of the data in the SES, which ran from 1988 to 1992 and was designed expressly for analyzing Senate elections. Relying primarily on the 1988 survey, Krasno confirms the work of other scholars that challenger quality has a significant impact on voters' decisions about congressional candidates. He also offers additional evidence about the importance of campaigns by comparing changes in voter attitudes toward incumbent senators in 1988 who subsequently ran in 1990.

In two separate chapters, Krasno provides evidence that voters make few distinctions between the job performance of senators and representatives and that the size and diversity of state populations have little impact on the electorate's evaluation of incumbent senators, except in the very largest states, whose senators nevertheless are quite successful in winning reelection. Having disposed of structural interpretations of Senate elections, Krasno then turns to respondents' perceptions of incumbent and challenger attributes. He uncovers patterns quite familiar to congressional scholars: Senate challengers are more visible to voters than House challengers, and high-quality challengers for both offices produce more intense competition than less experienced and visible opponents. Once Krasno controls for challenger quality and campaign intensity (themselves highly correlated variables), differences between other aspects of House and Senate races become negligible.

Although Krasno set out to explain variation in reelection rates of incumbent senators and representatives, his most important findings speak to the increasing similarity between members of the House and Senate—at least, in the minds of voters. Quite by accident, the SES study coincided with a spate of elections in which incumbent success rates for representatives and senators were quite comparable: for example, the relative reelection advantage for House members was 13% in 1988, −.9% in 1990, and 6% in 1992, which contrasts starkly with the 30-

point spreads of 1976–80 and the 20-point deficit in 1986. At the same time, House and Senate campaigns in the late 1980s began to look alike, as the number of million-dollar House races increased dramatically, and representatives professionalized their campaigns.

These trends are reflected in chapter 5, in which Krasno introduces successive sets of variables into logistic regression models of the vote for Senate and House challengers. In comparing the coefficients in the two types of races, he argues that campaign intensity is stronger in Senate elections than in House races, which in turn produces greater competitiveness. But this inference is faulty because examination of the coefficients' size is meaningless when two models have different Ns and variables with different means and variances. Indeed, when one examines the *relative* strength and significance of the variables in the two models in table 5.2, the results are almost identical.

Putting aside the similarities and differences of congressional races, some scholars might reject Krasno's analysis on the grounds that the SES data set is inappropriate for comparing Senate and House contests (see Abramowitz and Segal 1992). Survey research on congressional elections is always problematic because national samples, which give an accurate perspective on the House electorate, neglect voters in Senate races in small states. The SES compensated for this problem by oversampling small states, which of course provides an unrepresentative view of voters in House districts. Krasno addresses this point briefly in the appendix by noting that the SES sample contains a wide variety of House districts and that weighting the variables to compensate for the presence of so many small-state voters made little difference in the results. He also indicated that he had compared the 1988 SES data with the 1988 National Election Survey (NES) and that voter "perceptions of House candidates tracked very closely between the two surveys" (p. 174).

Krasno seems to have been less careful, however, in considering other possible limitations of the SES. For example, he ignores the controversy in the literature over how much the substantial overreporting of the vote for winning Senate candidates has biased the results toward incumbents. This is a critical omission, given his attempt to assess the strength of various influence on election outcomes. He also fails to consider whether the sample is truly representative of the competitiveness of Senate and House races in 1988. Given the perennial pattern in NES studies to oversample House districts with weak challengers and the tendency in medium-sized states to have low-intensity races, these aspects of the sample ought to have been checked more thoroughly.

Finally, anyone familiar with the literature on congressional elections cannot help but wonder at the absence of references to some of the major journal articles on Senate elections (e.g., Erickson 1990, 1990; Kuklinski and West 1981; Lewis-Beck and Rice 1986; Miller 1990; Stewart 1989; Squire 1992; and Wright and Berkman 1986). These omissions, when coupled with careless editing (e.g., the misspelling of Senator Arlen Specter's name, p. 12), raise questions in the reader's mind about the overall thoroughness with which the analysis was done.

The more significant oversight with respect to the literature, however, is the neglect of Canon's work on political amateurs in congressional elections. As it turns out, the years in which Senate incumbents have fared particularly badly have had disproportionately high percentages of successful amateurs. Such events run contrary to Krasno's main argument by suggesting that there may be more to Senate races than the intensity generated by high-quality challengers. Yet it is difficult to get at these other dimensions during the period under study because senators' reelection rates were unusually high.

On balance, Krasno makes a nice contribution to a growing body of literature on Senate elections. He enriches our understanding of the importance of individual candidates and campaigns in congressional politics and points scholars toward the issue of recruitment and the incentives that motivate strong candidates to run for Congress.

Dartmouth College Linda L. Fowler

With Reverence and Contempt: How Americans Think about Their President. By Thomas S. Langston. Baltimore: Johns Hopkins University Press, 1995. 180p. $24.95.

The era of the heroic presidency in political science may finally be over. The kind of veneration for the presidency that propelled the writings of a Clinton Rossiter or a Richard E. Neustadt is seldom found among the current generation of presidency researchers. Yet if a skeptical mood has become more common, it has not been given the scholarly elaboration it requires. Thomas S. Langston begins to fill this gap in *With Reverence and Contempt*, showing us why citizens and scholars alike need the presidency to be demystified.

Langston's book explicitly breaks with the behavioral approach to the presidency. He sides with "two trends in political science: the turn toward history and toward interpretation" (p. 3). In his hands, the study of the presidency is refocused on the meanings that the office has held in American political culture. Langston is also explicit about his disenchanted view of the presidency. "Each chapter," he writes, "examines a separate idea at the heart of a dysfunctional relationship. The people relate to their presidents and the presidents to the people in ways that impair the functioning of our polity" (p. 9).

If the presidency is largely constituted by the meanings that it holds for Americans, these meanings are, Langston argues, too numerous and too fabulous. He discusses six ideas in American political culture that link presidents and the people, offering historical examples and concluding, in each case, that the effect has been harmful for the public. Thus, he examines the president as head of the American civil religion (Lincoln, Wilson, Jackson, and Reagan), as champion of freedom (Lincoln, Truman, and Lyndon Johnson), as charismatic embodier of the people (Nixon and Reagan), as representative of democracy (Jackson and Kennedy), as invested with prerogative in crises (Jefferson, Lincoln, Franklin Roosevelt, and Bush) and the heroic presidency of political scientists.

Perhaps the most innovative and insightful of Langston's chapters is his discussion of the president as "a democratic avatar" (p. 73). He suggests that in the nineteenth century, "democracy" had a complex meaning: the "popular classes" sought greater equality through majoritarian rules and procedures. By contrast, the twentieth-century definition of democracy has retained only the procedural dimension. In the nineteenth century, only a few presidents (Andrew Jackson serves as Langston's example) could credibly claim to be champions of democracy. Today, every president adopts the mantle of democracy merely on the basis of his electoral success (John Kennedy is the example here), but the meaning of his service to democracy has become banal.

Langston's treatment of political scientists' role in presidential mystification is equally interesting, albeit more conventional. Briefly surveying and classifying the works of Neustadt, Burns, Barber, Lowi, Pious, Tulis, and Eastland, Langston implicates much of presidency scholarship in popular misunderstanding of the presidency. Presidents themselves have not, he argues, paid much attention to presidency scholars (with the exception of Neustadt), but "the popular expectation of heroic presidential leadership has been buoyed by political scientists who emphasize the personal and especially the moral aspects of their subject" (p. 131).

Langston concludes his book with prescriptions for "smashing the icon" (p. 135). The aim of his proposed reforms is to lessen the intimacy between the people and the president, to encourage the people to substitute respect for the skills of a politician in place of reverence for a priest-king (or contempt for a fallen idol). Thus, he favors proportional representation in the House, which might force presidents to manage multiparty coaltions, and a national council to take over the role of head of state. Another proposal is that symbolic prizes be bestowed on other leaders. (An unfortunate suggestion here is that famous Speakers of the House join presidents on our currency. Do we really want Newt Gingrich on the $100 bill?) But mechanisms of the sort that Langston recommends are, as he concedes, unlikely to gain much attention. If any headway is to be made against popular illusions about the presidency, it is going to take a sustained effort by political scientists and historians to teach students, journalists, and others to regard the presidency in a more modest light.

With Reverence and Contempt is a vigorous and engaging "polemic" (p. xiv) against the fables that shape American thinking about the presidency. It is a brief (144 pages of text) and highly accessible book, written in a breezy style—a good bet to spark lively classroom discussions. Most important, Langston has a thesis that presidency scholars need to consider—that the office is shrouded in myths that carry destructive consequences, paramount among them the political immaturity of the American people: "We are all familiar with persons who are passive yet unreasonably demanding. . . . They are our children. When it comes to our relationship with our presidents, we are too often the children" (p. 9).

One of the virtues of Langston's book—its combination of breadth with brevity—is also the source of its principal shortcomings. The critical arguments that he musters against the six myths about the presidency needed to be more fully developed to be more strongly persuasive. Similarly, the historical examples he offers are often so brief as to leave his critical points unclinched (and to leave the reader with the feeling that historical complexity and ambiguity have been slighted). Langston's book is the most instructive work we have so far for understanding how the presidency is constituted—and distorted—by American political culture. Yet it leaves much to be done, both analytically and historically. Future researchers in this field will owe Langston a debt for his mapping of the terrain.

State University of New York, Albany Bruce Miroff

Responding to Imperfection: The Theory and Practice of Constitutional Amendment. Edited by Sanford Levinson. Princeton: Princeton University Press, 1995. 338p. $59.50 cloth, $18.95 paper.

Few subjects of constitutional inquiry are more important than constitutional change. The reason, however, is not because of the intrinsic merits of change but because change is the natural way we moderns conceptualize responding to imperfections. "The world changes and so must the constitution as a *living* document" is the way the matter is typically put today. In other words, we look to the idea of change not, first and foremost, because we want or even expect the constitution to keep changing but because we want to keep the constitution (or whatever we regard as essentially constitutional about it), and we see change as the primary way of doing that. But how, when, and why do we change the constitution? And which constitution is to be changed—and by whom—in a federal democracy like the(se) United States in which federal and state governments have their own constitutions?

Cognizant of the thousands of times Americans have tried to change their federal and state constitutions over the past two centuries, the study of constitutional change becomes a daunting task. Mindful of the magnitude of this subject, Sanford Levinson has carved out a reasonable domain in *Responding to Imperfection* and succinctly states it in the book's subtitle. The book's orientation is more in the direction of theory than theory-and-practice, and its focus is primarily on the U.S. Constitution, occasionally in comparative perspective. But neither observation is as important as the fact that only 4 of the 13 articles in this book focus primarily on Article V and its amendments.

How can a book on federal constitutional amendments devote only 4 of 13 articles to Article V procedures and amendments? The plot thickens: one of the 4 debunks Article V, 2 are branded "exclusionist," and the exclusionists are defensive. Are we somewhere in Alice's Wonderland or book 3 of *Gulliver's Travels*? Actually, we may be in the world of *Forrest Gump*, where his mom says, "Forrest, life is like a box of chocolates, you never know what you're gonna' get."

Early in this book, Levinson writes, "Perhaps the simplest way of conceptualizing what we mean by an amendment is to describe it as a legal invention not derivable from the existing body of accepted legal materials" (p. 16). Later, he underscores this distinction: "There is no reason . . . to call a numbered textual addition a genuine 'amendment' unless it truly changes the preexisting legal reality" (p. 26). Holding aside *genuine* and *truly* (which never get operationalized), these statements clearly establish the "nonexclusionist" or "all-bets-are-off" view of Article V in the amendment process.

Constitutional life is, indeed, a box of chocolates with at least four choices: (1) numbered textual additions regarded by supporters, opponents, scholars, and jurists as the genuine article because they truly change the Constitution; (2) numbered additions that are not so regarded; (3) judicial interpretations regarded as virtually amending the Constitution; and (4) judicial interpretations that add nothing new to the Constitution. A fifth possibility, developed by Bruce Ackerman elsewhere and summarized in this book, is "the structural amendment" created during "a constitutional moment," (such as "the Radical Republicans' transformation of the Union in the aftermath of the Civil War" and "the New Deal Democrats' legitimation of activist national government in the aftermath of the Great Depression of the 1930s" (p. 67).

This is the basic matrix of choices (or can of worms) presented by Levinson both in this book and earlier in a series of American Political Science Association panels in which he brought the authors together over a four-year period to discuss constitutional change and the amendment idea. What do the authors do with this intellectual freedom? Roughly what one might expect of this group of constitutional law and political science professors: 12 quite different things. (Levinson writes two of the 13 pieces and is consistent throughout.)

Without trying to summarize 12 different arguments, I offer two sets of observations about the collection as a whole. First, the book's presentation bears little relationship to its apparent purpose. This book is a convenient collection of arguments made earlier and in greater detail by the authors. There is a certain freshness that comes from their interaction with one another, but the book is basically an abridged recapitulation and intended, one would think, for a wider audience. However, the articles are a curious mix of inside ball and grandstanding.

The first set of articles (by Stephen M. Griffin, Bruce Ackerman, Akhil Reed Amar, and David R. Dow) respond to the original matrix framed by the editor. Yet Dow bases his critique of Amar and Ackerman on articles by them not in this book. Next in order, Frederick Shauer writes about a somewhat different question: "What makes a constitution constitutional?" The next three contributions (by Walter F. Murphy, John R. Vile, and Mark E. Brandon) discuss the question, Are there substantive limits to constitutional amendments? The remaining three articles—Donald S. Lutz on American state constitutions and some national constitutions, Stephen Holmes and Cass R. Sunstein on constitutional revision in Eastern Europe, and Noam J. Zohar on the Midrash—are insightful and important comparative accounts, but each within its own carefully constructed theoretical framework. Taken independently, some of the articles are wonderful, and others are obscure; but they do not work as well together as they might have with more direction by the editor and self-discipline by the authors.

A final set of observations concerns the uses of history and other empirical evidence in this volume. In some instances, historical occurrences and written evidence are thoughtfully interpreted, but in others history seems to work its way in and out of the narrative as case law might be used to prove a point or substantiate a preconceived notion. A most welcome exception in this regard is Lutz, who carefully formulates his preconceptions into propositions to be tested. Yet no historians seem associated with this project. One historian, Richard B. Bernstein, has written a most thoughtful constitutional history, *Amending America* (1993); and he, for one, could have helped ground this collective endeavor without imprisoning it.

Russell Sage College STEPHEN L. SCHECHTER

The End of the Republican Era. By Theodore J. Lowi. Norman: University of Oklahoma Press, 1995. 275p. $22.95.

All of us who know Ted Lowi know that he is never timid or boring. His latest work, *The End of the Republican Era,* is cleanly and forcefully argued and filled with vivid conclusions.

It is also a pessimistic book—full of concerns and even forebodings. As he has for sometime now, Lowi sees much that is estimable in the American tradition ending, with no satisfactory replacements aborning. *The End of* is surely Lowi's favorite phrase. Long passages in this latest work are devoted to the end of the old liberalism, the end of the new liberalism, the end of conservatism, and, of course, the end of the Republican era. While *end* can and does connote different things here—including "goals"—Lowi's principal conclusions involve what he perceives to be the moral exhaustion or the democratic insufficiency of the several idea systems. The old liberalism (often referred to as classical liberalism) ran afoul of the maturation of the industrial order and was profoundly altered, without ceasing to be liberal, during the New Deal era. The philosophy that succeeded it and inaugurated the "second republic" degenerated into "interest group liberalism," ceasing to be a satisfactory public philosophy. A true conservatism, which Lowi sees having come to America only of late, is dismissed by the author as entirely unsatisfactory because he sees it fundamentally at war with democracy. The Democratic party lost its majority status with the exhaustion of the new liberalism; and the Republican party is likely, Lowi believes, to fail in building a national majority around conservatism. There is an end of the Democratic era, an end of the Republican era, the danger of an end of the *republican* era, and lots of grounds for hand wringing.

Lowi ends this book on a note that can only be called "mild despair":

> Even as a I compose these final passages, I already know that everything said here will be disregarded. I conclude feeling a kinship with Robert Browning's dying Bishop. After giving line after beautiful line of instructions to his sons for construction of his tomb, it begins to dawn on the Bishop that his sons . . . have gathered round him, bored and indifferent, just waiting for him to die.
>
> What is there about an ideology that blinds its believers to the limits of its applicability? Whatever it is, it makes tragedy the universal drama, in which politics imitates art. . . . Conservatism can never govern America because its view of authority is contrary to democracy: that there are persons who can know the truth and should, with the truth, govern. . . . The ultimate end of the republican era is therefore in the hands of liberalism, and what a dismal prospect that is. . . . The republic is too important to be entrusted to liberalism. But what else is there? (pp. 258–59)

Just over a quarter century ago, Lowi published *The End of Liberalism,* a work justly celebrated as a major accounting of the transformation of American ideas about governance. The new liberalism elevated government's role far beyond that of previous approaches, and a plethora of groups organized around the expanded state and bent it to their collective wills. Lowi's accounting of interest group liberalism remains persuasive because, while far-reaching theoretically, it was firmly grounded empirically. Just three years earlier, Grant McConnell described the same basic innovations in the American political institutional structure in an equally seminal work, *Private Power and American Democracy* (1966).

Lowi's subsequent work in the area is, in my view, far less persuasive than *The End of Liberalism,* because, rather than having a broad empirical foundation, it is rooted narrowly in his own idiosyncratic view of the world. The idiosyncratic character of *The End of the Republican Era* is seen in the author's treatment of conservatism. First, he argues that contemporary American politics has a new participant in the form of a true conservatism—"not the ersatz conservatism that is actually Old Liberalism erroneously labeled conservative but genuine conservatism—a substantive ideological confrontation with liberalism per se on the nature of government and the individual" (p. 109). But what Lowi calls conservatism (something very different from what Louis Hartz calls conservatism) has been an active player in American politics from the beginning of the republic. It has not been a warmed-over European conservatism—an ideological defense of ascriptive class principles. Neither has it been classical liberalism renamed conservative as further testimony to American exceptionalism. American conservatism has had roots in European liberalism but in other traditions as well, including a vital religious tradition that informed the Protestant Reformation and manifested itself in the early United States, notably in Puritanism. American conservatism—from day one, a blend of classical liberal assumptions about economic man and the limited state and a powerful infusion of religious individualism—has little intellectual kinship with modern European liberalism or what remains of aristocratic conservatism. Russell Kirk and others of like mind certainly sought to create an "old-order" conservatism in America, but they failed entirely.

Lowi thinks that the present effort to combine old liberalism and conservatism (as he understands it) under one roof is likely to fail. Here he echoes much current journalistic commentary that says that the Republicans will be unable to combine the "economic conservatism" "yuppies" and the like with the "social issue conservatism" of the "religious Right." This argument is, in a real sense, ahistorical.

There are always differences within any ideological "big tent." What we properly understand as conservatism in the American experience has always been an amalgam of the sort we see today. It may be that today's Republicans will be unable to manage this broad, distinctly American ideological tradition successfully; but the challenge they face in adapting it is hardly peculiar to our time. Politics involving the "moral dimension" has been prominent in the United States from abolitionism to populism to the temperance movement—and on to contemporary efforts by religious conservatives. It is deep-rooted, not a late-twentieth-century innovation.

Lowi worries about the contemporary manifestations of this tradition. He fears that the "religious Right" is fundamentally hostile to American democracy. I think he is wrong. What is more, I think he ignores an enormous amount of information—derived from actual behavior and from survey inquiries alike—demonstrating that the vast preponderance of America's religious conservatives are as deeply respectful of, and committed to, the nation's democratic traditions as is Lowi himself.

Lowi's personal forebodings, sincere and springing from a deep attachment to the American experiment, lead him to ignore a vast empirically demonstrated social/political reality. *The End of the Republican Era* lacks a broadly empirical accounting of the very sort that made *The End of Liberalism* so successful a work.

Roper Center for Public Opinion Research EVERETT CARLL LADD

Presidential Campaigns and American Self Images. Edited by Arthur H. Miller and Bruce E. Gronbeck. Boulder: Westview, 1994. 306p. $55.00 cloth, $19.95 paper.

It is rare to find a book that provides a truly fresh perspective on the well-covered territory of American electoral campaigns. Most studies proceed theoretically from the traditional descriptive, social-psychological, or rational actor perspectives. Research questions primarily address issues of campaign management, voting behavior, and the influence of campaigns on electoral outcomes. In addition, much work focuses on the independent roles of voters, candidates, and the mass media in campaigns, rather than considering the integrated influence of these three sets of actors on the process.

With this much-welcomed collection of essays, Miller and Gronbeck challenge social scientists fundamentally to reconsider the way in which they investigate political campaigns. They refute popular contentions that campaigns today are largely irrelevant because economic conditions predict outcomes. Instead, the authors argue that electoral contests are not simply referenda on policy and candidate preferences. Campaigns are an integral component of the American experience that reflect and help to create our cultural heritage. Presidential campaigns make fundamental statements about what Americans value, what they seek to reform, who they are, what they fear in the world, and who candidates hope they will become (p. 6). In short, elections are an essential part of the process by which Americans construct their political reality and develop their political self-image.

While there is no specific unifying theory that is employed throughout, the general theoretical orientation of the book derives from critical and cultural theories of human action that have a strong psychological component. The authors contend that citizens are active players in the electoral game, yet they take this argument further than most. In addition to assuming that voters react to, and process, political stimuli, Miller and Gronbeck assert that voters construct their own political world by attaching symbolic meaning and significance to economic indicators and other information they obtain from candidates and the mass media. The process of meaning construction itself has significant implications for American political culture. For example, this process influences the role that political parties play in elections during particular eras.

The authors stress the fact that it is essential to consider the role of candidates, media, and voters as part of a complex and integrated whole when studying campaigns. As such, the volume is organized around three major subthemes concerning image construction in presidential campaigns written by an impressive ensemble of authors from political science and communications studies. Each part is preceded by a brief introduction by the editors.

Part 1 explores candidate-generated images and includes chapters on negative candidate ads (Gronbeck) and images of civic virtue in political rhetoric (Lee). Morreale's essay on presidential campaign films from 1964 to 1992 is a standout in the volume. This piece takes a rare look at biographical campaign films that portray candidates' backgrounds and discusses the myths and images that candidates evoke through these films over time.

Mass-mediated images are considered in part 2. Two essays employing statistical analyses explore candidates and news coverage. Covington, Kroeger, and Richardson investigate candidate's media management strategies for attempting to control their images in the press. Woodard examines the outcomes of these strategies in terms of the amount of coverage given to candidates. Two thought pieces focus on debates (Birdsell) and media influence in campaigns (Rucinski).

Part 3—perhaps collectively the strongest—discusses images of the voter-citizen. Topics covered here are campaign polls (Merkle and Miller), how campaign messages become voter images (Boynton and Lodge), the symbolic role of social groups in campaigns (Miller), and money and politics (Bauer). Zullow provides an especially thought-provoking essay challenging the conventional wisdom that campaigns are occasions that showcase the most negative aspects of American politics. Instead, Zullow argues that presidential elections bring about a political rebirth by drawing attention to shared values and concerns that are a part of the tradition of "American exceptionalism." He provides empirical support for this view by demonstrating that candidates who present the most optimistic campaign messages capture the most public support.

For the most part, the essays are theoretically rich and intellectually stimulating. A good number include well-executed empirical analyses. However, one or two pieces do not meet expectations or fit as well with the theme. In particular, the essay on money and politics raises the interesting issue of the tension between freedom and equality where political spending is concerned. The essay, however, spends too much time recounting the history of campaign finance reform, which is widely available from other sources, and too little time addressing this important conflict. In spite of this limitation, it is still a useful piece, especially if the book is used as a class text.

One of the many strengths of this collection is that it emphasizes the ways in which historically derived images influence politics today. The essays demonstrate how American traditions, myths, and values are fraught with contradictions, ironies, and paradoxes that complicate the process of image construction. The authors conclude that the image of American culture conveyed via recent presidential campaigns is "an image that incorporates many contradictions between what ought to be and what is" (p. 262). These images are "characterized by high ideals and disappointing behavior" (p. 263).

In their conclusion, Miller and Gronbeck ask whether attaining democratic ideals is an impossible dream. They conclude that it is not but that solutions must take into account the interaction of all three sets of actors that are implicated in this dilemma. Candidates are supposed to uphold high moral and ethical standards, yet their campaign tactics frequently stoop to low levels. The mass media are supposed to fulfill a watchdog function but instead are manipulated by political officials and fall prey to the economic pressures of the news industry. Ideal democratic citizens are well informed and participatory, but in reality they are more tuned in to sensationalism than to responsible messages about politics and policies.

With this volume, the authors have opened an important door for future research. They have laid the theoretical groundwork upon which others can readily build. They provide concrete illustrations of the type of research that can contribute to a successful integrated and multidisciplinary approach to the study of campaigns from a cultural politics perspective.

Georgetown University DIANA OWEN

Reversals of Fortune: Public Policy and Private Interests. Gary Mucciaroni. Washington: Brookings Institution, 1995. 225p. $34.95 cloth, $14.95 paper.

What made tax reform possible in the mid-1980s? What accounts for the decline of anticompetitive regulation since the late 1970s? Why haven't various industries seeking greater trade protection during the 1980s been more successful? How have agricultural interests managed to escape a significant reduction in federal subsidies?

In *Reversals of Fortune,* Gary Mucciaroni answers these questions by systematically exploring for the fluctuating public policy fortunes of producer groups (i.e., industries, firms, and occupations that provide goods and services consumed by the public) at the federal level during the last few decades. He does so by developing a policymaking typology that juxtaposes the extent to which producer group interests are favored by the issue context with the commitment of institutional actors to resist or challenge producer groups.

This explanatory typology is used to analyze the fluctuating fortunes of producer groups involved in the policies of tax expenditures, deregulation, trade, and agricultural subsidies. Instances of shifting fortunes are analyzed in each policy area, yielding eight case studies from which Mucciaroni builds his case for attending to the role of issue and institutional contexts in the policymaking process.

The issue context is operationalized in terms of economic conditions, the kinds of policies currently in place (or not in place), and "symbols and images rooted in history and culture" (p. 15). Changes in the issue context create new opportunities for policymaking coalitions, which in turn influence the attitudes and behavior of institutional actors. The institutional context is operationalized in terms of executive branch autonomy, the strength of congressional committees, and the extent of committed leadership. As these variables change, so will the policy fortunes of producer groups. Producer group influence is less a function of the groups themselves, Mucciaroni argues, than it is contigent upon changes in the "issue contexts and institutional arrangement," which "can either facilitate or impede groups' activities and influence in the policymaking process" (pp. 26–27).

All of the policy decisions studied by Mucciaroni involved concentrated benefits and diffuse costs. This is the key to understanding how the issue and institutional contexts determine policy outcomes. Policies, such as corporate tax credits or restrictions on competition, "have diffuse, externalized costs" but also "very few obvious diffuse benefits." Consequently, they "have little support among the public" (p. 169). When the issue context becomes unfavorable for a producer group (as during a period of economic growth), "it is more likely that policymakers will resist policies with concentrated benefits and diffuse costs, that they will launch challenges against those that already exist, and that they will succeed in their efforts" (p. 165). Equally important (and a better predictor of fluctuating fortunes in the eight case studies) is the favorability or unfavorability of the institutional context, which Mucciaroni consistently elaborates.

This is a well-written, carefully documented, and thematically persistent volume. Mucciaroni seriously considers how changes in context of issues (such as economic conditions) can change the perception of a policy and hence the politicking that follows. He also emphasizes the importance of informal as well as formal aspects of institutions for the determination of policy outcomes. Moreover, this book contains numerous insights about policymaking and politics in America. Contrary to a view held by some students of interest groups, "the main source of countervailing power vis a vis entrenched economic interests has been actors in government rather than in society" (p. 181). Contrary to popular belief, a Congress more open and democratic actually increases the opportunity for interest group access and influence (p. 89). Consistent with other recent studies, Mucciaroni finds that "clientelism is no longer" an applicable characterization of American politics (p. 181).

This volume also has a few weaknesses. First, it largely ignores the influence that producer group activities and those of their hired lobbyists may have on policymaking. Chapter 5 systematically examines the case for an explanation of policymaking based in producer groups. Here, Mucciaroni argues persuasively (if too briefly) that group mobilization and resources along with group size and exclusivity of membership are contingent and weak explanations respectively for policy fortunes. The presence of organized opposition can be consequential but is not decisive. Producer groups have the most influence over policy fortunes due to the potential for coalition building. However, unlike the analysis of group activity in *Organized Interests and American Democracy* (1984) by Kay Scholzman and John Tierney or the detailed case studies in *The Lobbyists* (1993) by Jeffrey Birnbaum, Mucciaroni says little about the role of "raw political pressure" in the determination of policymaking, except to note its general lack of importance (p. 10).

Second, insufficient attention is given to the dynamics that shape the specific interests, incentives, and proclivities of institutional actors. These actors, along with the formal and informal characteristics of the institutions within which they serve, play a major role in Mucciaroni's explanatory model. We know institutional actors are influenced by the issue context. However, the conditions that make possible autonomous or semiautonomous behavior by institutional actors, along with the processes that shape their interests, and so on are never fully explicated. This is a problem that Mucciaroni shares with other state-centered studies of policymaking.

Third, many readers will finish *Reversals of Fortune* only to wonder about the applicability of this approach to the analysis of policies that entail different concentrations of costs and benefits or different kinds of organized interests. For reasons explained early on, Mucciaroni focuses exclusively on policies where the benefits are concentrated and the costs diffuse (pp. 186–87, n. 10). However, producer groups are also actively involved in policies where both costs and benefits are concentrated and where costs are concentrated and benefits diffuse. Moreover, there are a wide assortment of policymaking situations in which there are being allocated by the state "costs" and "benefits" that do not involve producer groups at all. Applying and refining Mucciaroni's explanatory model to these additional cases of policymaking is an important challenge for future research.

Scholars studying interest groups, agenda building, congressional politics, the effects of institutions on policymaking, and these specific policy areas will find *Reversals of Fortune* valuable and instructive. So, too, might the public. For *Reversals of Fortune* is a direct rebuttal to the popular claim that a plethora of special interests in America "are petrifying the government," leading to what Jonathan Rauch has called *Demosclerosis* (1994).

Mucciaroni does not find "a government stalemated or overloaded by competing interests" (p. 182) nor one disabled by relentless rent seekers. Policymaking in America is admittedly "more atomized and chaotic," but a "more individualistic politics" also affords "leaders greater maneuverability" (p. 183). Whether America's leaders utilize that maneuverability wisely and whether the electorate has anything to say about its use remain questions about the nation's fortune beyond the scope of Mucciaroni's persuasive analysis.

University of California, Irvine MARK P. PETRACCA

The Fictional Republic: Horatio Alger and American Political Discourse. Carol Nackenoff, New York: Oxford University Press, 1994. 364p. $35.00.

Carol Nackenoff has written an engaging, wide-ranging, and sympathetic study of Horatio Alger's contributions to American political discourse in the late nineteenth century. Disputing the conventional wisdom, which simply dismisses Alger as an apologist for emergent industrial capitalism in America, Nackenoff persuasively argues that to Alger, "change and tradition are reconcilable; character, virtue, power, independence, and individual agency can triumph in the age of capital" (p. 268). According to Nackenoff, Alger was wedded to New England Whiggery. He was therefore distinctly uncomfortable with both the opulence and the squalor that infused the Gilded Age.

Primarily, Alger was a writer of popular fiction directed mainly to adolescent boys. Nackenoff effectively describes the plot formula Alger developed: a rite of passage in which a "manly" boy from humble rural origins encounters and overcomes the evils of the city, retaining his honesty, compassion for the weak, diligence, artlessness, and optimism. Eventually the hero is rewarded with modest enrichment by benefactors who recognize his merit. Alger compounded this formula from many influences, including his Unitarian roots, the advice manuals that flourished in the nineteenth century, the fiction of Dickens (whom Alger admired), and his observations of street life in New York. From this welter of experiences, Alger constructed a "guidebook for survival" in the new world of industrial capitalism (p. 53).

Nackenoff treats this formula not only as a means by which Alger achieved popular literary success but also as a political statement. In so doing, she takes a bit of literary license herself, seeing more in Alger than he might himself have recognized. Nevertheless, she makes a virtually seamless transition from the fictional formula to its imputed political meaning, avoiding the labored prose to which many practitioners of cultural studies have recently fallen prey.

For Nackenoff, Alger is, above all else, a republican. Indeed, his fiction is an allegory of American republicanism. The hero's successful passage from youth to adulthood parallels the successful passage of the republic from birth to maturity. Despite the increasing perils it faces, the republic, like an Alger hero, survives because it preserves its fundamental virtues.

What are these virtues? Alger's fictional republic protects productive property and rejects financial speculators, confidence men, and "capitalists" who simply move money around. Alger's republic is egalitarian in manners, eschewing invidious distinctions between social classes. Alger's republic despises artificiality, legalism, spectacle, and pretension, favoring plain human virtues, such as forthrightness, modesty, and perspicacity. Alger's republic celebrates commerce over corporate industry and prefers frugality to materialism and prodigality.

Alger is a republican but not a democrat. His fiction, his letters, and his public statements are replete with fears that the public, left to its own devices, becomes not only unenlightened but also gullible. Alger worried that an unholy alliance might form between unscrupulous elites—skilled in deception and seduction—and the new mass public entranced by the false promises of the Gilded Age. The republican America whom Alger cherished was desperately in need of enlightened leadership and uplifting literature, such as his own. We cannot fail to see in Alger faint resemblances to Veblen and the early progressives.

Nackenoff attempts to treat Alger's fiction as partly a form of resistance to industrial capitalism and the political corruption of the Gilded Age. But this interpretation does not wholly persuade. By Nackenoff's own admission, Alger ignores most of the dominant features of the new economic and political order. In his stories, there is little wage labor and little factory work. Nor are there political machines and their bosses, labor unions, or immigrants. Nackenoff points out that Alger condemns greedy squires and preying "tramps," but these were hardly the real dangers of his age. How can one resist that from which one deliberately averts one's eyes? If Alger's heroes never directly challenge the new centers of power, they are more irrelevancies than champions.

Alger believed that "character" (i.e., self-discipline, hard work, honesty, and self-reliance) would continue to exert its beneficial force in the world of corporate capitalism, trusts, and political machines. But he never properly tested this proposition in fictional encounters between his heroes and the new holders of power. Indeed, Alger clearly underestimated the negative effects of the new environment on the formation of personality. Perhaps the honest newsboy can withstand the occasional greedy squire, tramp, or con artist. But can he defy—much less reshape—the grinding daily regimen of the factory, the anonymity and danger of the metropolis, the force of the machine? Henry Adams stared at these new realities unblinkingly and wrote an American classic in his *Education;* Alger blinked, and became a sentimentalist.

Ironically, Alger's fiction fell into disrepute within his own lifetime. By the 1870s, many of his moralistic critics were condemning his stories as sensationalistic and trashy. Meanwhile, literary intellectuals, caught up in the vogue for realism, derided Alger as simplistic. Though squeezed between the bluenoses and the literati, Alger shouldered on, promoting his work where he could, desperately holding onto a reading public. Nevertheless, a certain poignancy slips into Nackenoff's account of his later career. By the mid-1880s, "Many publications failed to note his new works at all or simply scoffed" (p. 225), and by the World War I, Alger's books had disappeared from many public libraries.

Does Alger, then, have any lasting political relevance? Nackenoff believes he does. The "enduring appeal" of Alger, she argues, is that he is "*adaptable* to a variety of American political and cultural conflicts" (p. 266, emphasis original). Perhaps so, but one wonders if adaptability to so many points of view renders him irrelevant to any single one. Fortunately, Nacknoff's sensitive, undogmatic analysis leaves plenty of room for the reader to form his or her own opinion about Alger's ultimate impact on American political culture.

University of Wisconsin, Madison RICHARD M. MERELMAN

The Imaginary War: Civil Defense and American Cold War Culture. By Guy Oakes. New York: Oxford University Press, 1994. 194p. $29.95.

A fascinating moment in cultural, social, and political history is here evoked in this highly readable book by a professor of Philosophy and Social Policy at Monmouth College. Oakes states his thesis with admirable clarity. He would analyze "the relationship of national security and civil defense to civic ethics in the early years of the Cold War" (p. 7). He presents a single extended argument around the question, How could we keep a country united by World War II but exhausted at its prolonged scope and cost keyed to "cooperative endeavors required for survival in the nuclear age"? and the answer, "Civil defense" (p. 4). Civil defense was precisely what it sounds like—an alarmed but proud and peaceable public rallying to defend itself against a menacing foe and an even more menacing weapon, the atomic bomb.

It was a delicate business. People had to be sufficiently frightened out of their wits to sustain the "long, twilight struggle"—in JFK's stirring phrase—but not so discombobulated they flinched and went for appeasement, or "peace at any price," as it used to be called. The "uncertain variable" here was "American national will," described by Oakes as "always a fickle and unpredictable entity for foreign-policy makers" (p. 7). But if you believe, as America's great foreign policy elite did, that national security depended on the robustness of the American character, then that amorphous entity, the national will, must be aroused yet channeled. The will of a democratic people is a sleeping giant, after all, and too much arousal does as much harm, potentially, as too little.

Oakes may overdo the prescience of foreign policy makers and domestic emotional managers or their ability to manipulate and fine-tune as bumptious an entity as "the American people"; but for those of us who remember 1950s childhoods, there are a number of shocks of somewhat bemused recognition as one turns the pages. Civil defense became a moral obligation, a way to demonstrate concern and public spiritedness. Bottled water in the cellar; duck and cover drills in elementary school (I don't think we ever did that in Timnath, Colorado, for it was too insignificant a place by far for the Russians to mean to do us harm); school assemblies with local civil defense volunteers passing out pamphlets on how to survive fallout. Oakes traces the connections between public policy, propaganda, marketing, and public ethics. He insinuates his points, rather than bludgeoning you with them.

By reminding us that the Cold War can't just "go away"—all that pent-up mobilization must find a home somewhere, he suggests—he does a useful civic service. In fact, if the Cold War was anything, it seems to have been a version of the "moral equivalent to war" of a sort William James would have found problematic, although a number of progressives of the World War I era (many of whom were keen on the "nationalizing" and "common good" aspects of war making) probably would not. For indeed, the more ardent architects of the Cold War were imbued with a quasi-French Revolutionary ethos of a nation mobilized toward one shared goal, a *levée en masse* of a unified people. Oakes cites an editorial in *Colliers* magazine opining that "civil defense involves the active instantaneous participation of every able-bodied man, woman, and child" (p. 57). America's peacetime ethic could not be a "soft" ethic: "A hard school was required, in which Americans would learn to practice in peacetime the more stringent virtues they needed to avoid war" (p. 23). In my own work, I have called this version of public life "armed civic virtue"; and Oakes's book is a fine illustration of it or of the attempt to build and sustain it. Civil defense militarized domestic life. We were armed rhetorically and "repeatedly admonished" that we "must respond like soldiers in battle—good soldiers, naturally, who follow instructions competently, automatically, and unflinchingly so that the objectives of their commanders can be achieved. Civil defense militarizes life by nonmilitary means, using techniques of emotion management in order to train Americans to manage themselves" (p. 77).

Did Americans pass the test? Rather well, Oakes concludes, although trying to make Americans march in unison is rather like attempting to herd cats. The improvisational nature of the civil defense effort is intriguing. Policymakers were flying blind, so to speak, making stuff up as they went along. I don't mean "inventing a threat" (it was real enough, and the release of materials from Soviet archives underscores the point); but improvisation was the order of the day. Who was responsible? For what? Should we reassure the people that they can be safe if they take precautions or give them the fullest possible picture of just how devastating an all-out strategic nuclear strike would be? Oakes calls these the "minimizing" and "maximizing" strategies. Civil defense wound up as a "therapy of choice," a way to inform people about danger but beat back panic. Training for civilian defense was not unlike training to be a soldier, planners determined, for repetition of doable tasks was the key—hence all the drills and pamphlets and propaganda films.

I have one caveat and two observations that intrigued me and are worth further pondering. I did not recognize Oakes's representation of George Kennan as a man filled with "disdain for the ordinary American" and riddled with "contempt for the quality of American public life" (p. 25). Kennan is an aristocrat, to be sure, but he is a very *democratic* aristocrat of the Jeffersonian sort and deserves better than this.

Now to the observations. First, Oakes hints that the 1950s was, in fact, a powerful staging ground for feminism. As early as 1950, "The Office of Civil Defense Planing . . . took a general position of women's role in civil defense that corresponded roughly to their view of contemporary liberal feminism: given the same training, men and women could do the same jobs" (p. 141). Women played a major role in the entire effort. The most effective spokesman for civil defense was, in fact, a rather remarkable woman, Katherine Howard, an Eisenhower appointee. Second, Eisenhower himself emerges as the tough old war-horse that he was, a man with few illusions about what nuclear war would mean. Oakes details some of Eisenhower's "disconsolate reflections of the fallacies of the Cold War conception of nuclear reality," and it makes for fascinating reading (p. 158). He rejected any preplanning for a preemptive nuclear attack as "contrary to American traditions" (p. 159). He anticipated the most "abysmal consequences," arguing that the imagination "could not encompass" the reality of a nuclear war and declaiming, "War no longer has any logic whatsoever" (p. 163). Vice-President Nixon, too, is credited by Oakes with a "ruthless" (what else?) and "clear-sighted" dissection of just what a nuclear war would cost in human lives. The United States could not survive, he argued in a National Security Council Meeting on 27 March 1958, toward the end of the high point of the "duck and cover" era (pp. 166–67). Just a few years later a new administration took over and a high-octane era under the hyperkinetic JFK began, one that brought us closer to the brink of nuclear war than anything under Eisenhower, even as civil defense fervor faded.

University of Chicago JEAN BETHKE ELSHTAIN

City-Building in America. By Anthony M. Orum. Boulder: Westview Press, 1995. 261p. $65.00 cloth, $19.95 paper.

Anthony Orum employs the method of comparative case analysis to build a general argument concerning the developmental trajectory of American cities. He devotes more than half his book to the history of Milwaukee since its inception in 1818. Then he develops brief, highly schematic accounts of Cleveland, Austin, and Minneapolis–St. Paul, in order to identify those aspects of Milwaukee's development that follow a common pattern. Finally, he presents a stage theory of urban development that encompasses both structural and idiosyncratic (or conjunctural) factors. He models his argument on Ann Markusen's *Profit Cycles, Oligopoly, and Regional Development* (1985), contending that cities pass through a set of stages analogous to the profit cycle of firms, as they move from founding through maturity to decline.

City-Building in America thus has two aspects: a highly textured, quite fascinating history of Milwaukee and an exercise in theory development. Both merit careful reading. Orum traces the development of Milwaukee from its creation in speculative land development through its heyday as an industrial metropolis. He emphasizes the role played by the early entrepreneurs, who exercised almost total control over the city's fortunes and who constituted the only real governing authority. He argues that the period of industrialization was marked by three significant trends: (1) rapid population growth, (2) institutionalization of the economy as control passed from individuals to industrial corporations, and (3) expansion of the municipal government, marked by the rise of a salaried civil service and full-time politicians. In chronicling the industrial period, he notes the irony that one of the unique aspects of Milwaukee's history, its long domination by a social democratic government, ended not in the empowerment of the working class but in the empowerment of city government. As in the "red" cities of Europe, socialist rule brought Milwaukee an efficient government that fostered business prosperity. Finally, Orum identifies the factors that contributed to the out-migration of people and jobs from post–World War II Milwaukee, resulting in a city with a marked decline in its standard of living, seriously disadvantaged in relation to its suburbs, and afflicted by racial conflict.

Orum contends that the trends he analyzes in Milwaukee represent general tendencies that characterize other cities at the same stages of their development. Cleveland, founded at roughly the same time as Milwaukee and possessing a similar industrial history, exhibits almost identical patterns of growth and decline. Even Austin, despite apparently glaring differences, is following a similar course, albeit in a different era. Thus, both Austin and Milwaukee began as the product of individual entrepreneurs and later became dominated by institutions. Both saw the rapid growth in power of municipal government at a time of economic and population expansion. Likewise, Minneapolis and St. Paul displayed similar cycles of population and industrial growth and shrinkage. The Twin Cities are an anomaly, however, in that, despite their midwestern location and industrial past, they resemble sunbelt cities in having retained a viable downtown and an educated population working in business and financial services. Orum attributes this difference to an economic base that always had a major service component, as well as to greater social homogeneity that was itself partly a consequence of its industrial make-up.

On the whole, Orum makes his argument persuasively. We can see clear stages of urban development, particularly among the old manufacturing cities of the heartland. Whether or not we can view the development of sunbelt cities as following a precisely similar set of stages, however, remains open to dispute. Austin, in its stage as a mature city, has a different economic base and different politics from Cleveland and Milwaukee in their analogous periods. The latter were industrial cities at their peak; Austin is a mature service-dominated city. Equating the two conditions is highly problematic. Orum himself mentions that one important difference between Austin and the other cities he discusses is the determining role played by the federal government in Austin's expansion. Essentially, federal water, military, and technology policy, rather than private investment capital, acted as the trigger that set off Austin's explosive growth. Moreover, Austin does not suffer from suburban strangulation since it has been able to annex its hinterland. Milwaukee and Cleveland could not do likewise, in part because of their large black populations.

These differences suggest significant dissimilarities between sunbelt and old industrial cities that are likely to have important future implications. Pointing to a common maturity, indicated by a large and relatively autonomous municipal government, may obscure the continuing contrasts between urbanization in the southwest and in the rest of the country.

Orum's analysis, while locating Austin's good fortune in federal policy, tends to ignore the role of the federal government in affecting the stages of urban development. Although he discusses the enormous impact of the New Deal on Milwaukee, he does not demarcate his stages of development according to changes in the federal presence. Consequently, in his concluding discussion of ways in which the decline of Cleveland and Milwaukee can be halted, he focuses on the internal politics of these places. Yet it is only the federal government that has the resources and regulatory power to redress the disadvantages suffered by the residents of old, central cities. Orum intends to be a realist by not looking to Washington for remedies. But in taking this position, he ignores the relationship between local and national politics that springs from racial conflict, the urban/suburban divide, the widespread depiction of urban populations as unworthy of national assistance, and the general effect of the Republican party's agenda. It is hard to imagine an effective, progressive urban policy in the absence of a national effort.

Although at times, Orum presses his thesis too tendentiously, in his conclusion he makes a summary statement that captures the complexity of urban development: "Each and every American city is constituted both by the intersection of institutional development and stage, and the historical conjuncture in which it arises. Thus, differences arise among cities because of the variability both of structural development and of specific historical circumstances" (p. 201). This recognition of both structurally induced similarities and important differences among cities provides the basis for useful theoretical generalizations, as well as for detailed case histories. *City-Building in America* thus makes a welcome contribution to the growing body of theoretically informed comparative studies of cities in the United States and abroad.

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SUSAN S. FAINSTEIN

Constitutional Domains: Democracy, Community, Management. By Robert C. Post. Cambridge: Harvard University Press, 1995. 331p. $45.00.

A spate of recent books on the First Amendment free speech clause has appeared, inspired largely by debates about regulating pornography and hate speech. They range from advocacy to systematic constitutional interpretation, and occupy every point on the (notoriously confused) ideological spectrum of unregulated speech/enlisting government in censorial control. On both of these matters the U.S. Supreme Court has remained largely unmoved by this scholarship. This is not to say that the turbulent political debate is purely academic, but the constitutional theories advanced are noticeably so to the extent that they rest on analytic categories drawn from moral and political philosophy: "autonomy", the "harm principle", and so on.

Robert Post's book is a contribution to the academic debate, also written from a standpoint outside of doctrine. But the author, a professor of law, is steeped more deeply in social theory than moral philosophy. His interest is in "patterns of rules that establish recognizable forms of 'social ordering'." (1) And his aim is less to criticize judicial reasoning or promote a particular role for the court than to comprehend how we got the messy law we have. It is elegant, illuminating, displays virtuosity in analyzing opinions, and is theoretically satisfying. And it deals with fresh materials. (Post uses tort law, the *Hustler Magazine* v. *Falwell* case, and public forum doctrine extensively to illuminate speech law generally.) Knowledgeable about contests among scholars in the field (e.g., he admirably sorts out the harms ascribed to racist speech), still, Post does not spend too much time wrestling with the usual cast of First Amendment commentators. He has his own story to tell.

Post argues that constitutional law defines three distinct social domains with distinct purposes (the realms of community, democratic public discourse, and government management) and that constitutional principles embodied in the laws of each domain have their own logic and integrity.

Laws regulating speech in the domain of community, whose purpose is the preservation of a common cultural identity, uphold and enforce shared rules of civility and norms of appropriateness. The common law torts of invasion of privacy and public disclosure, blasphemy, and the constitutional law of libel and defamation are examples. They cannot be understood simply in terms of personal injury, on this view, and must be seen as protection of social personality and of the special claims that community members have on each other, as distinct from others.

The assumed norms of community life are suspended, however, when the court defends the domain of debate about public affairs, whose purpose is to facilitate communication (and competition) among persons of varying traditions and cultures and preserve a space for the advocacy of new forms of community life. This expansive freedom for public discourse, Post argues, is based on reasons that depend not on choosing between the conflicting goals of individual autonomy and collective self-government but on their inseparability.

In the third domain—government's managerial authority over its internal institutional operations—speech regulations are conditions for achieving instrumental objectives. (Interestingly, for Post, the value of speech in public universities turns on neither community norms nor democratic self-governance but on the logic of instrumental rationality and thus depends on the nature of the educational mission we attribute to these institutions.)

This typology helps makes sense of a great deal. Post uses it to sort out the "twisted and unhappy history" of public forum doctrine, for example. Inexplicable if viewed in terms of a geographical location, tradition, or the character of government property at issue, the "underlying pattern" of public forum doctrine becomes comprehensible in terms of the distinction between managerial authority and governance authority over the general public. And contemporary doctrine aims less at defining public forums and protecting public discourse, Post observes, than at protecting "nonpublic forums," that is, demarcating a class of public resources in which public expression claims are devalued and managerial authority upheld. When routine judicial oversight will interfere with effective institutional functioning, judicial deference to managerial roles preserves the boundaries of this social order.

As this suggests, the drama comes as the boundaries of these domains are defined, conflict with one another, and are renegotiated: it is struggle between a realm of public discourse and an organization domain of instrumental rationality—for example, between the common law's resistance to the enlargement of the public sphere as inconsistent with community identity and the nonenforcement of the norms of established and stable groups in public discourse. The contribution of Post's structural analysis emerges in his deft explication of key clashes and his sober reminder that conflicting legal judgments are "explicable as an ongoing process of dynamic, contextal, and practical accommodation" (p. 196).

At present, scholars are most preoccupied with the clash between civility rules and public discourse. It is the "paradox of public discourse" that processes of collective self-determination presuppose social cohesion—some comprehensive community that makes constitutionalism possible, hence a modicum of civility and respect. That said, Post emerges as strong proponent of the court's jurisprudence of the last 60 years, which insulates public discourse from existing communal standards and the constraints of orderly processes and which, he recognizes, will be experienced by some not as ensuring access to democracy for those who would be excluded if censored but as licensing outrageous behavior. Here he diverges sharply from collectivist democratic theorists, among them Meicklejohn and his heirs Cass Sunstein and Owen Fiss, who are willing to regulate and abridge public discussion in various ways so that it serves specific ideals of rational public deliberation. Post links the value of autonomy here to the fact of social heterogeneity, to the indeterminacy of the content of democratic discourse at any time, and to the conditions for "responsive democracy," that is, the continuous formation of "future communities."

This fine work raises three sorts of questions, beginning with whether Post's account has applicability outside of First Amendment speech law. There are, after all, other "social orders"—among them, markets and intimate private life. Do Post's domains pretend to explain substantive due process or the commerce clause? What does his structure say about the Second Amendment? The reach of his constitutional theory is unclear.

The book also raises questions about the relation between this theory and judicial decision making. Plainly, Post does not think his structural analysis is transparent or that it is explicitly used to justify opinions. The business of defining and redefining boundaries is latent in the overall course of the law; Post is a hermaneuticist discerning its "internal logic." Yet the court is the agent here, and Post proposes that his analysis is not just explanatory, illuminating what is implicit, but should be used to guide doctrine. Is this what makes "constitutional domains" more valuable to a court inclined to take the guidance of scholars than, say, the moral accounts of Dworkin? Or is the force of

Post's prescriptions his preference for a particular kind of democracy?

In the same vein, though Post's main object is to explain us to ourselves, there is the normative question whether these three domains are peculiarly American and whether they (and not just their mobile boundaries and the allocation of conduct to one or another) are a justifiable way of differentiating social orders for us?

In short, this book on freedom of speech is vivid, original, and challenging in surprising ways, providing a coherent account of the particular tensions, doctrinal muddles, and overall course of U.S. speech law.

Brown University NANCY L. ROSENBLUM

The Politics of Problem Definition: Shaping the Policy Agenda. Edited by David A. Rochefort and Roger W. Cobb. Lawrence: University Press of Kansas, 1994. 216p. $29.95 cloth, $14.95 paper.

David Rochefort and Roger Cobb are well known to scholars interested in policy development and the policy process—Cobb for his truly groundbreaking work with Charles Elder on agenda setting in *Participation in American Politics* (1983) and Rochfort for his theoretical contributions to the role of images in the movement of issues onto policy agendas in *American Social Welfare Policy* (1986). In this edited volume, they combine their perspectives to examine one of the most important aspects of the policymaking process—problem definition.

The book consists of an introductory, integrative chapter by Rochfort and Cobb; seven chapters illustrating various aspects of the politics of problem definition through detailed policymaking case histories of issues, including plant closings, air transportation, sexual harassment, drug use, tax policy, agricultural policy, traffic congestion, and AIDS; and a final chapter by Christopher Bosso placing the study of problem definition in a contextual perspective of culture and structure.

Rochfort and Cobb describe the function of problem definition in political discourse as being "at once to explain, to describe, to recommend, and above all, to persuade" (p. 15). In the first chapter, they bring together several streams of scholarly literature which treat problem definition as important: social conflict and politics, social construction of reality, post modernism, and policy analysis. At their core, all these approaches to problem definition emphasize how the uses of language are crucial to understanding the dynamics of public policymaking and how—at the level of definition—social problems are extraordinarily malleable.

In their individual chapters, the case study authors employ at least some of the dimensions of claims about problem definition that Rochfort and Cobb set forth in the introduction as particularly likely to be invoked when social problems are set forth for the governmental policy agenda. These include causality (e.g., Is the problem a result of individual lack of effort or a failure of the economic system?), severity (How serious are the problem and its consequences?), incidence (How many people are affected? Is the problem growing, and if so, at what rate? How long are people affected?), novelty (To what extent is this a "new" problem?), proximity (To what extent is this a problem that seems "close to home" to people?), and crisis (To what extent is this a problem to which the rhetoric of crisis can be applied?). In the case studies, we see that the more a problem can be shown to be the result of system failure, to have serious consequences for large numbers of people, to strike close to home, and to have dimensions of a crisis, the more likely it will attract attention and rise in salience on the governmental policy agenda. The case studies show in action Cobb and Rochfort's argument that objective conditions surrounding a condition must be understood as inextricably related to the words used to describe them.

The authors of the case studies deal with diverse topics and employ different methods to examine the politics of problem definition. The most empirically interesting is that of Frank Baumgartner and Bryan Jones who use the *Readers' Guide to Periodical Literature* to retrieve and examine articles from 1900 to 1985 in order to analyze the changing definition of the issue of air transportation. The case studies are both fascinating and instructive, and many attempt to address some "puzzle" in understanding problem definition. For example, in her case study of the issue of sexual harassment, Ellen Frankel Paul shows that a problem that has been around for some time without much visibility needs to gain reinvigoration through a "crystallizing moment" such as a dramatic incident, a catastrophe, or a scandal that will cause the national media spotlight to shine on it. For sexual harassment, Paul describes the "earthquake" moment as the Anita Hill—Judge Clarence Thomas case, and the "aftershock" incidents as the Tailhook case and the disclosures regarding the reported indiscretions of Senator Packwood.

Elaine Sharp describes the much older policy problem of drugs and notes that the contemporary war on drugs is simply the latest in a number of episodes of policymaking directed to the drug problem dating back to 1914. Her chapter asks what keeps this problem alive. According to Sharp's analysis, durability requires two things: problem characteristics that allow transformation of the problem and thus lead to novelty (e.g., the continuing debate about whether the drug problem is caused by evil drug traffickers or by the psychological weaknesses of individuals) and political actors who have a stake in sustaining public interest in a problem.

In a comparison of the problem definitions in the cases of tax policy and agriculture policy, Gary Mucciaroni continues and expands the exploration of causality in problem definition begun by Sharp, picking up on Rochfort and Cobb's point in the introductory chapter that how a problem gets defined often depends upon where the cause of a problem is located and who gets assigned the blame. According to Mucciaroni's analysis, the different fates of tax reform and farm policy reform reflect the way the groups benefiting from the policy were portrayed as well as the way the problem affecting them was defined. Tax reform succeeded because large business corporations and very high income families were portrayed as receiving unfair advantages in the tax system at the expense of low-income and middle-income Americans, while farm policy reform failed because farmers were portrayed not as receiving unfair advantages but, rather, as being victims without blame for their plight.

The crucial role of values in the politics of problem definition is a continuing theme in the case studies. Rochfort and Cobb argue that "value concerns drive the process of public policy making in all fields" (p. 163), and in the AIDS case they make that value clash come alive. In a case study of traffic congestion as a public problem, Joseph Coughlin examines how individualist versus communitarian values dominate American political conflict in determining problem definition that involves the use of common resources such as the nation's highways.

In the final chapter, Christopher Bosso attempts to set the politics of problem definition seen in the previous eight chapters in societal context by drawing on two of the perspectives set forth by Rochfort and Cobb in chapter 1: the Schattschneiderean conflict model and the social construction of reality model. Using material from the seven cases, his thoughtful and insightful analysis shows how "societal characteristics and cultural values converge with existing structural and political conditions to create the contexts within which political actors jockey to promote competing problem definitions and formulate public policy" (p. 200).

This edited volume is a useful addition to the literature on policy development and the policy process and will be used now and in the years to come in both undergraduate and graduate courses on policymaking. No other volume exists about the politics of problem definition that is so packed with both good insights and interesting cases. Yet the reader is left with a sense of frustration at the quite messy portrayal of the politics of problem definition that emerges, at the editors' failure to impose much of a structure on the whole, and at the case study authors' few references to each others' chapters. Nonetheless, Christopher Bosso says in his final chapter that the entire policymaking process simply *is* "sloppy and complicated" (p. 201) and that the best inroad to understanding the *hows* and *whys* of change may be a better understanding of problem definition. Certainly, this volume is a good start; but one hopes that in their *next* book Rochfort and Cobb will provide a better map and a fuller development of patterns to help us wend our way through this very important aspect of the policymaking process.

Northwestern University FAY LOMAX COOK

Public Entrepreneurs: Agents for Change in American Government. By Mark Schneider and Paul Teske, with Michael Mintrom. Princeton: Princeton University Press, 1995. 263p. $39.50.

What is public entrepreneurship? It is, first of all, an alertness to opportunities for personal gain in the public sector—power, celebrity, personal achievement, perhaps more than financial gain. It is the ability to discover unfulfilled needs or create new demands and then risk reputation, time, and energy in pursuit of change. It requires the ability to motivate and coordinate networks of individuals and organizations.

We used to call this political leadership, but in modern public choice theory, it is much more rigorously defined. And *Public Entrepreneurs* is heavy with theory. Indeed, Austrian economics and neoclassical economic theory provide the starting point for the sophisticated development of a theory of nonincremental political change based on the role of public entrepreneurship. The "relaxation" of the strict assumptions of the neoclassical model of the marketplace—perfect information, low entry barriers, absence of transaction costs, product homogeneity, multiplicity of buyers and sellers, and resulting equilibrium disturbances—opens the door to entrepreneurship. It is entrepreneurship that produces *innovation*, the "the most extreme form of entrepreneurial alertness and discovery" (p. 44), as well as *leadership* (the tapping of latent demand and the shaping of preferences) and finally *arbitrage/adaptation* (the borrowing and adopting of ideas from somewhere else). Entrepreneurs must be willing to bear risk to their reputations, credibility, and careers, as well as costs in time, energy, and lost opportunities. They must be able to overcome the familiar collective action problem to communicate, motivate, and coordinate the actions of others.

Perhaps the most important contribution of *Public Entrepreneurs* centers on the development of a theory of the market for public entrepreneurship—the identification of the conditions and institutions that affect the supply and demand for community entrepreneurs—and the subsequent empirical testing of this theory. The testing is admittedly crude: an examination of the results of 953 usable responses to mailed questionnaires sent to city clerks in about fourteen hundred suburban municipalities of one hundred large metropolitan areas. But the authors "passionately believe that theoretical advances must be routed in *empirical* tests of the propositions embodied in that theory," even though abstract theories are "more elegant and more rigorous than our ability to gather empirical data for testing" (p. 213).

The clerks in about 30% of these communities nominated someone as a public entrepreneur—an individual in their community whose "policy proposals and political positions represented a dynamic change from existing procedures." These entrepreneurs—most often mayors or managers—were then classified as progrowth or antigrowth. (Regrettably the questionnaire is *not* reproduced in the text or appendix of the book.) Various institutional, demographic, political, and fiscal conditions in these communities are regressed against the presence or absence of progrowth or antigrowth entrepreneurs. Twenty-six separate tables report findings about everything from institutional forms of government, the strength of parties and taxpayer and neighborhood groups, type of elections and electoral districts, racial composition, home ownership, growth, income, tax rate, types of expenditures, and so on. The results are modest yet intuitively satisfying. A somewhat tangential, theoretical discussion of the Tiebout model is later thrown in for good measure.

To provide relief from all of this heavy theorizing and all of these multiple regression problems, the authors thoughtfully provide 10 boxed, short case studies of community entrepreneurship.

Overall, *Public Entrepreneurs* is an impressive piece of scholarship, worthy of close attention by students of urban affairs. It can be read profitably for its theoretical contributions alone, even if the mailed questionnaire results are predictable. And the case studies are insightful and entertaining. It is an important contribution to the literature on political leadership, innovation, and change.

Florida State University THOMAS R. DYE

Abortion Politics in American States. Edited by Mary C. Segers and Timothy A. Byrnes. Armonk, NY: Sharpe, 1995. 279p. $55.00 cloth, $19.95 paper.

In recent years, a number of books have addressed the volatile area of American abortion politics. Therefore, it is important to ask what another volume contributes to this literature. According to the editors, there were two major goals for this book: to describe the contemporary politics of abortion in the United States and to explain the differences in abortion law across the United States by using case studies (p. 12). Segers and Byrnes point out that given this policy area, it is necessary to look at the states where much abortion policymaking has, and continues to, take place. If *Roe* (1973) federalized abortion politics and policy, then *Webster* (1989) refederalized it: "In 1992, *Casey* continued the refederalization process" (p. 8).

Case studies from 10 "representative" states are presented in separate chapters. States were chosen for their geographic diversity, as well as for the varied approaches that they took before the *Roe* v. *Wade* decision. Toward those ends, California, Washington, and Arizona are included from the West; Minnesota and Ohio from the Midwest; Louisiana and North Carolina from the South; and Pennsylvania, Massachusetts, and Maryland from the East. Contributors were asked to consider contextual factors (e.g., public opinion and state political culture) in general and four political factors in particular: the strength and effectiveness of abortion-related interest groups, the views and activities of the state's governor, the nature of party politics within a state, and the particular role played in abortion politics by the state's female legislators" (p. 12).

There are also chapters on the national right-to-life and prochoice movements, which were supposed to discuss how each has responded to the "refederalization" of abortion, as well as a concluding chapter on the future of abortion politics in the American states. The editors note that all but two of the chapters were written for this book and that no chapter has been published previously.

This book does make a contribution to both the abortion policy and state politics literature by showing the wide diversity in state policymaking. Many of the state chapters are fascinating. Without the case study approach, important facts and historical details would not have emerged. For example, Washington voters approved a state initiative in 1991 to secure access to abortion. North Carolina created a state abortion fund in 1977 to offset the effects of the Hyde amendment. In 1992, Arizona voters defeated a constitutional referendum to limit abortion. In her chapter on Ohio, Patricia Bayer Richard included the results of interviews with 56 state legislators during 1990 and 1991. This chapter is a masterful combination of qualitative and quantitative research.

The chapters, however, are uneven. Quite simply, there is considerable variation how and whether the authors addressed the variables suggested by the editors. Another complication is the variation in time frames. Ruth Ann Strickland's chapter on North Carolina manages to address all of the editors' interests: it looks at the situation from the mid-1960s (pre-*Roe*) to 1993, analyzes interest groups, party politics, the governor's role, and the influence of women legislators. Other chapters focus almost exclusively on important events that occurred within the span of a single year, such as Maryland's 1992 referendum on abortion rights.

Two chapters address the respective roles of national interest groups after the *Casey* decision. While it is interesting, the prolife chapter does not take a broad view of the movement and all of the constituent groups that comprise it. It gives short shrift to the increasing violence; instead, it somewhat defensively emphasizes Governor Casey's consistent positions, as well as the "common ground" approaches tried with prochoice groups in several localities. The prochoice chapter is much better organized and far more successful in discussing a broad-based national movement in the wake of *refederalization*. Unfortunately, the chapter is somewhat dated now because it ends with Autumn 1993 events, such as implications of the Clinton health care plan.

The case studies themselves do offer a fascinating glimpse into the role of organized religion in state politics. Of particular interest is Christine Day's discussion of the influence of Catholicism in Louisiana abortion politics. It is not nearly as simple as the absolute proportion of the population Catholic. After all, Louisiana has about the same percentage of Catholics as do New York and New Jersey. In his chapter on Arizona, Daniel O'Neil examines the difficulty of forging a Catholic-evangelical alliance, even where both interest groups may agree on abortion policy. In the Massachusetts chapter, Mary Anne Borrelli argues that the Roman Catholic Church has recently experienced diminished political authority in state politics. Here I agree with the editors that the case study technique provides more insight than would many aggregate analyses.

However, the study of politics should not be a matter of one approach or another. The goal is to understand and explain and even to set the stage for future research. In the conclusion, "The Future of Abortion Politics in the American States," Byrnes points out that case studies can inform aggregate analysis and says that "at the least, we have offered a road map for the study of abortion politics in the American states" (p. 262). This volume, as a whole, falls far short of that potential.

The dissimilar foci of these case studies, as well as the lack of uniformity in the definitions of concepts simply do not lead to general conclusions. For example, even while acknowledging that political culture is "a very blunt explanatory instrument," Byrnes concedes that "many of our contributors have used the term *political culture* very loosely as synonym for political context or political setting" (p. 251). However, in the conclusion, he uses "the term much more carefully," restricting it to Elazar's definition. Clearly, the cohesion of the book would have improved if this type of uniformity had been imposed on the contributors at the beginning of the project.

University of New Mexico DEBORAH R. MCFARLANE

Legislators, Leaders, and Lawmaking: The U.S. House of Representatives in the Postreform Era. By Barbara Sinclair. Baltimore: Johns Hopkins University Press, 1995. 329p. $39.95.

In a major—and more important—complement to her 1983 book, *Majority Leadership in the U.S. House*, Barbara Sinclair has made a significant contribution to our understanding of the conditions under which activist party leadership occurs in Congress and the ways in which it has operated. It is a complement to her earlier book, because it extends the rich analysis of the developing party leadership activities that were still in experimental stages at that time. It is more important, because of its theoretical grounding, its completeness, and in its testing of whether leadership involvement makes a difference in legislative success.

Prior to this work, there existed two sources of explanation for the growth in party strength in Congress: the external ones and the internal ones. The former relied mainly on an analysis of factors that led to more homogeneous parties in Congress. Credit was given largely to the long-term implications of the Voting Rights Act for changing the nature of southern Democratic representation in Congress and, to a lesser extent, to the growing activities of the national parties in the recruitment, training, financing, and packaging of House candidates. The impact of these outside forces was well documented in a number of places, particularly David Rohde's *Party Leaders in the Postreform House* (1991). But although many congressional scholars have written about the internal explanations and the various vehicles that were placed in the hands of party leaders to mobilize their members, the extent of the impact had always been open to question. Did they give the party leaders the capacity to make their membership more cohesive and to influence legislative outcomes? Or was all this a function of having a more homogeneous membership in the first place? In sum, how were the external and internal explanations for stronger congressional parties linked, and what influence did each play?

Through the use of principal-agent theory, Sinclair is able to make a well-ordered link between the internal and external explanations. Viewing the majority party leaders as agents and the rank-and-file House members as principals, she argues that a more homogeneous Democratic House membership decreased the costs of stronger leadership and that the institutional changes created by the reforms of the early 1970s increased the benefits that stronger party leadership could provide. With a more ideologically homogeneous membership, there was less chance that collective outcomes the leadership might pursue would be detrimental to a sizable segement of members. And stronger leadership could protect desired legislation from being unraveled in a more broadly participatory House or from being defeated by a polarizing opposition-party president. Thus, a major strength of Sinclair's book is the application of principal-agent theory to the existing substantive knowledge. No doubt, some of these ideas are alluded to elsewhere, especially that dealing with the tension between the goals of individual members and the collective goals of the institution, but Sinclair has integrated them in an especially thorough fashion.

The body of the book analyzes how House Democratic party leaders gradually used the greater authority entrusted in them from the late 1970s into the early days of the Clinton administration. This includes chapters on organization, communication, and accountability; on the whip system; on the Rules Committee; on prefloor activity; on assembling floor majorities; and on setting the agenda. Some of this material is drawn from previous work by Sinclair and others but not with the degree of thoroughness, subtlety, and richness of illustrations that Sinclair provides. In reading these chapters, I was struck by two lessons, in particular, that the material suggests. Sinclair is explicit about the first of these. It is that the party leaders now place far greater emphasis on managing an entire legislative program, rather than just thinking in terms of individual pieces of legislation. One problem with much congressional research (be it case study material, rational choice analysis, or roll-call analysis) is that there is a tendency to see pieces of legislation in isolation and treat the players as if nothing else is of concern than the given item at hand. Sinclair, on the other hand, lets us see that party leaders have concern with the bigger picture. They recognize that resources are limited; that using two extra days on bill X becomes two days no longer available at the end of the session for bills Y and Z. Legislation does not just compete for majorities to ensure passage. It also must compete with other legislation.

Second (and implicit) is how "professionalized" the congressional process has become in the House. The repertoire of strategies and tactics available to the majority party leadership is quite impressive, and the members expect the leaders to produce results for them as individuals and as a party. It struck me that the legislative process that Sinclair reveals is not at all like the "sausage-making" analogy of conventional wisdom. There is a good deal more planning, skill, and concern about the end product than the sweat, brute force, and policy bastardization that are the conventional wisdom's lore.

Importantly, Sinclair is able to demonstrate that active leadership involvement has a marked effect on committee success both on the floor and in terms of the final outcome. Her claim that it has a bigger effect than whether the president opposed the committee position may be stretching the data a bit (p. 299). But I know that no other study has empirically demonstrated the effect of leadership involvement.

I would be remiss in commenting on this otherwise outstanding work if I did not mention two reservations. The first is that since appropriate individual credit is given for leadership initiatives (afterall, this book will be of historical value and not just for an audience of present-day legislative scholars), it strikes me as odd that Richard Bolling's name is only mentioned once and then not in regard to the changes in leadership powers. True, Bolling left the House in 1982, but he was responsible for placing many of the mechanisms in place upon which congressional leaders a decade later rely. The use of the Rules Committee to structure choices was largely a Bolling invention that formal party leaders had to be persuaded to use. It was Bolling who offered the motion in the Democratic Caucus to give the Speaker the power to nominate the party's Rules Committee members and urged the Speaker to reject a state delegations claim to pick a member. O'Neill, Wright, Foley, and others made their own major contributions. But in looking at the entire era, it strikes me that Bolling was the critical figure in ensuring that party leaders had the tools with which to operate. And in certain areas such as committee reorganization and ethics reform, he tried to go beyond what the majority party and some its leaders were willing to support.

Second, Sinclair deals all too briefly with the shortcomings of House majority-party leadership in the postreform era. Although there is useful discussion of how a Republican majority leadership will operate, there is almost no examination of whether the Democratic House leadership bears any responsibility for the 1994 debacle. In particular, the ethical shortcomings of a number of those in major leadership positions, the failure to react to changing public expectations for higher ethical standards, and the reluctance to provide appropriate leadership in this area strikes me as a fundamental weaknesses of an entire era of Democratic House leaders. From the rejection of the Obey Commission recommendations to the House bank scandal to Speaker Foley's stock market success, the personal and institutional leadership shortcomings deserved some systematic treatment. They were too numerous and continuous merely to be idiosyncratic.

In sum, however, this book is a most remarkable undertaking. It represents a scholarly research effort that has stretched over nearly two decades. It has involved the getting and maintaining an unusual level of access to the workings of Democratic party leadership and yet an ability to remain sufficiently distant to develop the broader perspective. The end result is clearly a most important—if not the most important—book on party leadership and Congress.

Vanderbilt University BRUCE I. OPPENHEIMER

The Case against School Choice: Politics, Markets, and Fools. By Kevin B. Smith and Kenneth J. Meier. Armonk, NY: Sharpe, 1995. 167p. $55.00 cloth, $19.95 paper.

Americans have a long tradition of treating schools as if they should be—and for the most part are—outside the messy realm of politics. Political scientists know better, but they have succumbed in at least one respect. The bland and apolitical language with which educational issues typically are framed in the national dialogue rarely catches the interest of political scientists, who thrive on conflict and the assertion of power. The discipline, as a result, has ceded this dialogue to educational professionals and the lay public. One exception was during the late 1960s and early 1970s, when battles over desegregation and community control over schools brought racial and class conflict out into the open. Currently, as debate about school reform is increasingly linked to issues of educational "governance," political scientists seem, again, to be pricking up their ears.

The focus on educational governance results from broad frustration with the failure of public school bureaucracies to respond with sufficient vigor to problems evident in many of the nation's classrooms. Advocates of more radical school reform argue that meaningful changes in policy and performance cannot take place until the institutions of educational governance are redesigned. This argument frequently is tied to the call for educational vouchers and increased school choice. The most influential articulation of this position is John Chubb and Terry Moe's *Politics, Markets, and America's Schools* (1990).

In this book, Smith and Meier take direct aim at the institutional theory that undergirds the position adopted by Chubb and Moe. Most critiques of Chubb and Moe's work have focused on their empirical comparison between public and private schools; but Smith and Meier zero in on central premises about the relationships among democracy, bureaucracy, and performance. Chubb and Moe's institutional theory presumes that democracy necessarily promotes bureaucratization and that bureaucratization necessarily interferes with educational performance. In contrast to public bureaucracies, vouchers and other school choice programs will encourage schools to improve by subjecting them to competitive market pressures. Smith and Meier contrast this with a perspective holding that democracy does not hinder performance, that bureaucratization is a function of needs in the external environment (not a function of democratic control), and that competition promotes segregation and elitism.

The heart of the book, then, comprises three distinct empirical tests of the institutional theory, each carried out using a different level of analysis. The first test utilizes district-level data from all public schools in Florida. Student performance, the dependent variable, is measured by standardized tests of math and communication skills. Competition is operationalized as the size of private school enrollments in the district (an indication of the viability of the exit option) and the percentage of students enrolled in gifted classes (conceptualized as an indication of the public schools' competitive response). Bureaucracy is operationalized as the number of school officials per student. Democracy is operationalized as the presence of an elected (versus an appointed) district superintendent. Counter to the prediction that democracy impedes performance, an elected superintendent was as likely to be associated with higher test scores as with lower ones. The greater presence of private schools as an exit option was associated with poorer performance; Smith and Meier attribute this to cream skimming, which leaves the public schools to deal with students with greater educational disadvantages. Consistent with the institutional theory, they find that more bureaucracy is associated with poorer performance. Rather than a necessary consequence of democracy, however, they find that higher levels of bureaucratization are negatively associated with elected superintendents.

The second set of empirical tests uses the 50 states as the unit of analysis and includes both cross-sectional and longitudinal data. Here the dependent variable is a standardized index based on states' average Scholastic Aptitude Test and test scores. States that have adopted a statewide open enrollment policy—an indicator of school choice—are found to have poorer performance, while adoption of some of the more conventional reforms that choice advocates disparage (e.g., teacher certification and mandatory graduation tests) is associated with better test scores. The presence of an elected head of the state educational system is strongly associated with higher test scores, counter to the institutional theory prediction that democracy impedes performance. As at the district level, the state analysis finds some support for the prediction that bureaucracy hinders performance. States in which nonteachers constitute a higher percentage of the public school employees had somewhat lower scores.

The final empirical test looks at cross-national differences. This is necessarily a much less systematic analysis, since truly comparative data is not readily available; but like the others, it raises questions about the basic premises of the institutional theory. Critics of the U.S. educational system, including school choice proponents, frequently hold out Germany and Japan as examples of the way that strong schools ultimately can bolster national economic performance. Great Britain is cited by choice proponents as an example of a country that has formally adopted a more choice-based approach. Smith and Meier's analysis points out the highly centralized and bureaucratic elements of the German and Japanese educational systems. If these two nations do a better job of educating their children (Smith and Meier explain why we should be wary of the claim that they do), it is difficult to see how this can be attributed to decentralization, competition, and school choice. Their review of the British experience highlights some of the problems and political conflicts that have become apparent in the implementation stage.

The somewhat rash subtitle of this book gives the false impression that this might be an ideological diatribe. Rather, it proves to be a valuable effort to demonstrate that good social science can elevate public dialogue about policy issues by isolating key assumptions and putting them to empirical test. Sharp-eyed readers may find those tests unsatisfying in some respects. In particular, the operational indicators are too narrow and indirect fully to capture such weighty and multidimensional concepts as democracy, bureaucracy, and market choice. *The Case against School Choice* illustrates the welcomed engagement of political science in an important national debate. Undoubtedly, it does not mark the final word.

George Washington University JEFFREY R. HENIG

Wilsonian Idealism in America. By David Steigerwald. Ithaca: Cornell University Press, 1994. 296p. $37.50.

As the century draws to a close, political thought in Western democracies is in turmoil. Fascism, communism, and socialism—the great "isms" that defined much of twentieth-century political life—are in disrepute. Liberalism stands triumphant in an industrialized world undergoing rapid technological transformation. But it is a liberalism that is increasingly uncomfortable with itself. Questions about the welfare state, regulatory reform, and revived ethnic nationalism have caused many today to reexamine what liberalism has become and where it is going. Such a reexamination inevitably involves understanding the complicated past that is liberalism in general and American liberalism in particular.

David Steigerwald's first book, *Wilsonian Idealism in America,* is a welcome study of an important tradition of American liberal thought. Political theorists have long recognized the influence of a Wilsonian perspective in American political life. But they have often confused it with other strains of liberal thought ranging from Croly nationalism to conservative internationalism to New Deal liberalism to Kellogg-Briand pacifism. Steigerwald is sensitive to the complex history of liberalism in American and weaves a thoughtful tale of how a group of moderate American liberals attempted to sustain a faith in reason throughout a turbulent century.

The book works on two levels. On one, Steigerwald traces out the birth, evolution, and ultimate demise of a tradition of liberal political thought. He recognizes that this tradition may not be as self-defining as others. Some of the protagonists might rightfully reject the labels "Wilsonian" and "idealism." Nevertheless, he argues that Wilsonian idealists accepted a common set of assumptions about the world and took up a certain temperament toward the world that set them apart from other liberal thinkers. Wilsonian idealists believed that modern economic relations had undermined laissez-faire capitalism and that modern science was displacing the traditional republican values that had defined much of American political life. But such beliefs did not shake their faith either in the free market or in a free people's being able to govern their own affairs. In opposition to the factional forces of pluralism, Wilsonian idealists appealed to a "reinvigoration of a sense of disinterested statesmanship devoted to the common good" (p. xi).

On a second level, Steigerwald charts out the backgrounds, careers, and ideas of a group of influential political and cultural elites that played major roles in American political life from World War I to the end of the Cold War. These individuals range from well-known public figures like Woodrow Wilson, Walter Lippmann, and Adlai Stevenson to lesser known lights such as Elihu Root, Nicolas Murray Butler, Raymond Fosdick, and Newton Baker to contemporary thinkers such as Patrick Moynihan, Francis Fukayama, and Theodore J. Lowi. One of the great strengths of the book is the way in which these individual stories are woven neatly into a larger discussion of an evolving tradition of political thought.

The book begins with an analysis of the emergence of Wilsonian idealism out of the trauma of World War I. Following a discussion of the academic and political career of Woodrow Wilson, Steigerwald explains how a group of influential elites came to see politics from a Wilsonian perspective. For these people, many of the problems of the nation and the world were a result of conflict-ridden factional politics brought on by the unenlightened pursuit of self-interest in the modern world. They saw an interdependency among the nations of the world that could not be ignored, and viewed internationalism and enlightened statesmanship as a way to sustain liberal idealism in the postwar world.

The history of Wilsonian idealism chronicled by Steigerwald is less one of triumph than of ongoing disappointment and disillusionment. Far from embracing the tenants of Wilsonian idealism, American politicians and the public turned to other political persuasions for solutions to the nation's problems from the 1920s to the 1960s. Wilsonians found themselves pushed back into a brand of cultural elitism trapped on the horns of a dilemma that they were never to escape. On the one hand, they came to blame the problems of the nation and the world on social and cultural pathologies that ran deep. On the other hand, they had trouble questioning their basic faith in some form of public reason. The debacle of the Versailles Treaty was only the first of many challenges that threatened Wilsonian idealism. Isolationism, pacifism, New Deal liberalism, and Cold War liberalism came to define mainstream American politics in a way that Wilsonian idealism never could. Steigerwald does an excellent job explaining what was attractive about Wilsonian idealism to cultural elites in America and why, in the end, it fell into disrepute in the hands of overrated intellectuals like Adlai Stevenson.

The book is not without its limitations. Steigerwald carefully analyzes how evolving notions of internationalism and global interdependency helped to redefine Wilsonian idealism as a political tradition. But with the notable exception of an excellent discussion, in chapter 6, of the response of a few individuals to the New Deal, there is little analysis of what might be termed the domestic political economy of Wilsonian idealism. Did

Wilsonian idealists articulate a vision of the domestic political economy that evolved coherently over time as their view of internationalism did? How did Wilsonians in the 1920s look upon the War Industries Board of World War I? Did this perception change over time? Did Wilsonian idealists like Stevenson in the 1950s agree with their predecessors of the late 1930s that the domestic programs of the New Deal were evidence of a growing nationalism that threatened international prosperity and peace?

Addressing such questions might have compelled Steigerwald to broaden his criticism of Wilsonian idealism. It may be that the problems with Wilsonian idealism lie less in its faith in public reason or its naive understanding of human nature than in the way its displaces a concern over national affairs for those of a hypothetical international community. True statesmanship may demand recognizing that benevolence for the average person is a limited commodity that cannot easily be extended to include the nation-state, much less beyond it. For better or worse, democracy may begin and end at home. Any brand of liberalism in the next century that fails to remember this simple fact runs the risk of walking in the footsteps tread by Wilsonian idealists for much of the twentieth century.

University of Texas, Dallas EDWARD J. HARPHAM

The Rebirth of Federalism: Slouching toward Washington. By David B. Walker. Chatham, NJ: Chatham House, 1995. 384p. $29.95.

The Rebirth of Federalism is a follow-on and update of David Walker's 1981 book, *Toward a Functioning Federalism.* Walker concludes in his new book that the American federal system is still not functioning properly. The system is somewhat less centripetal than it was in 1980 but still nation-centered, still marked by "creeping conditionalism and galloping social regulation" (p. 7), still experiencing an erosion of federal-state tax comity, and "still overloaded, not quite as much as it had been in 1980 but still overloaded" (p. 311). These conclusions, however, leave one puzzled by the book's title and asking, therefore, "Where's the rebirth?" Given the author's conclusions about the nation-centered state of the federal system, a rebirth of federalism would presumably entail a shift of power to the states and, perhaps, their local governments as well. Yet, as the book's subtitle declares, power is still "slouching toward Washington," which is not the Bethlehem toward which the Devil slouches in William Butler Yeats' poem "The Second Coming."

Written in Walker's inimitable no-holds-barred style, the book offers a panoramic overview of the political history of American federalism from the founding era to 1994. As a current text, the book will find a prominent place in courses on American federalism and intergovernmental relations. The book is thick with descriptive detail, providing plenty of information for classroom discussion and student papers, though it is thin on theoretical road maps. As the descriptive detail thickens in its coverage of contemporary federalism, the author seems conflicted about the nature of the forest produced by the trees and concludes, in the final analysis, that the system is conflicted itself. The contemporary federal system does not, in the author's view, exhibit a clear developmental future. There are conflicting directional signals, despite the "slouching toward Washington" metaphor.

The principal theme of the book, therefore, is the growth of the power of the federal government from 1789 to the present. Walker provides a good discussion of key decisions of the Constitutional Convention relating to federalism and of various understandings of federalism during the founding era. Left aside, however, are the covenantal roots of American federalism and the debate over the "republican" character of the founding, both of which are significant for interpreting the origins of American federalism. Walker then traces the development of federalism through two eras of dual federalism (pre– and post–Civil War), an era of cooperative federalism (1930–60), an era of mixed "conceptual offsprings" of cooperative federalism (1960–80), and an era of reaction to cooperative federalism (1981–present). The new federalism of each post-1789 era is described thoroughly, although the rationale for designating the periods is unclear.

The eras also have two curious characteristics: (1) each new era is shorter than the previous one, and (2) the eras correspond to calendar decades. A theory of accelerated social change might account for increasingly truncated eras, though not for decade consistency in timing. One result is that the scheme lacks predictive potential and cannot anticipate what would appear, from the book's implicit logic, to be the start of another era with the Republican gains in the November 1994 elections, congressional enactment of the "Unfunded Mandates Reform Act of 1995," and the U.S. Supreme Court's April 1995 Tenth Amendment ruling in *United States* v. *Lopez,* which held, for the first time since 1936, that the Congress exceeded its constitutional authority to regulate interstate commerce when it criminalized under federal law the possession of a gun within one thousand feet of a school. Indeed, the substantial federalization of criminal law since 1968 is barely addressed by Walker.

The burden of the argument of the book is the author's call for a rebirth of "an authentic federalism" (p. xiii) to solve the problems of a political system "that is overloaded, seriously confused, and distrusted" (p. xii). An authentic federalism would feature more prominent governance roles for state and local governments and a better balance of power between the federal government and state and local governments. A difficulty with the argument is that the author does not specify what an authentic federalism would look like. A rebirth also suggests a restoration of something lost, but no golden age of federalism is highlighted as a model for a contemporary rebirth. Left unclear, therefore, is how an authentic federalism would resolve contemporary problems and how it would do so without resurrecting problems that led to the downfall of previous eras of federalism.

Walker presents a sizable and worthy agenda for reform entailing (1) a stronger constitutional basis for federalism, (2) strengthened state and local party organizations, (3) heightened representation of state and local government interests in federal government institutions, especially the Congress, and (4) a "Big Swap" of domestic responsibilities along the lines proposed by Alice M. Rivlin in 1992 (*Reviving the American Dream*). Given that these reform ideas are familiar to students of American federalism, it would have been useful to go beyond their summary presentation in order to spell them out more specifically, assess their feasibility, and analyze their possible consequences. For example, Walker proposes that "Article V's state-initiated amendatory option needs to be sanitized so that it actually may be used" (p. 319). However, there is no analysis of recent proposals for amending Article V, including one adopted by the National Governors' Association in 1988 at the initiative of then Governors John Sununu and Bill Clinton.

Not given much attention are some significant emerging issues affecting the future of federalism, particularly (1) the technological information revolution, which poses difficult questions of intergovernmental regulation and taxation of the technology itself, as well as issues concerning the impact of information technology on power sharing, and (2) free-trade agreements, such as GATT and NAFTA, which have corrosive implications for state and local powers of regulation and taxation.

These critical points, however, do not outweigh Walker's valuable and fairly comprehensive contribution to the literature on American federalism, especially his keen grasp of the fine points and nuances of the contemporary federal scene.

Lafayette College JOHN KINCAID

Black Politics and Black Political Behavior: A Linkage Analysis. Edited by Hanes Walton, Jr. Westport, CT: Praeger, 1994. 416p. $59.95 cloth, $19.95 paper.

Black Politics and Black Political Behavior is one of the most ambitious and theoretically driven volumes of collected works about African Americans that has recently appeared in the social sciences. Editor Hanes Walton, Jr., is to be commended for assembling a powerful set of essays that cohere theoretically but range over topics as wide as gender and black politics, the institutionalization of black social movements, the relationship between black politics and national and state political institutions, and the not-always-honorable history of how the discipline has used and abused both research and researchers of black politics. At the same time the volume (and Walton's editorial direction is very evident here) makes four major theoretical points. First, one cannot undertake quality research into black politics, Southern politics, or indeed American politics without understanding the centrality of not only racial conflict but of black politics as a key element of racial politics. Second, such research can not be competently done unless one understands the historical and social context and continuities to be found in black politics. (This point is aimed at a theoretical quantitative analyses in general and survey research of a certain type in particular.) Third, one can competently conduct such research or understand black politics unless one has a knowledge and appreciation of the early pioneers in the field such as Ralph Bunche who without acknowledgment provided much of the key research for what the discipline sees as the seminal works of V. O. Key and Gunnar Myrdal. Finally, qualitative studies—particularly but not exclusively comparative case studies—can provide the type of nuanced descriptive research that provides the material for theory building and against which deductive theories can be tested. This last point has a corollary: most, not all, survey research and virtually all research in the "race relations" tradition fail to provide useful or even valid findings because of their methodological and theoretical weaknesses. While this last point is one that some of us many wish to debate, the book makes critical contributions by showing us the rich historical and continually important legacy of the early black pioneers in black political research. Further, the book strongly makes the point that many of us too easily forget that even in the contemporary period the serious study of black and indeed American politics must be solidly grounded in, and understanding of, the politics of race and the regional politics of the South.

As is usually the case for volumes of this type, the research chapters vary in quality. However, in this case they range from short pieces that provide useful descriptions of unexplored aspects of African-American politics to penetrating essays that make theoretical arguments supported by solid empirical material. One of the unifying themes of the volume is that black politics should be viewed historically as a struggle for justice and against the Southern (and in some essays) national system of white supremacy. The central theoretical theme in the manuscript is the linkages of black citizens to the state, to indigenous political organizations (e.g., the black church, independent black parties), and to other African Americans through group consciousness and group identity. There are exceptionally rich research chapters by, among others, Walton, Holmes, and the team of McClemore and Coleman on the linkages between African Americans and the state. One important example is provided through several essays by Walton that document the importance of black voting in the South during periods where blacks have been assumed by many to be politically quiescent through institutions ranging from local elections to New Deal farm programs. McClemore's and Coleman's analysis of the historical antecedents of the Jackson campaigns and their efficient use as a mobilizing device by Jackson is another excellent chapter that sheds new light on a series of campaigns that have been scrutinized recently by many political scientists but not from this vantage point. Cheryl Miller's chapter on black legislative behavior in North Carolina provides solid evidence on how black legislators represent black interests differently from—in some ways perhaps represent black interests better than—their white counterparts, a subject of recent intense debate. Miller's chapter and others emphasize another theme in the volume—black politics has historically had to operate under severe constraints. As Walton shows us, in Mississippi, blacks can still have elections systematically stolen from them by the courts and other agencies even in the current period.

Other theoretical themes in the book are more problematic. Rickey Hill's challenging essay on rethinking the paradigm that governs black politics posits a *single* black experience driving a single set of black issues. This is an assumption that is being justifiably challenged within the black community by intellectuals and scholars such as Toni Morrison, Cathy Cohen, and many others. Indeed, perhaps the greatest absence to be found within the entire volume is a lack of commentary and research on the political (both individual and institutional) consequences of difference and division *within* African American communities. The work of those who work in the critical race and gender legal studies (e.g., Kimberle Crenshaw, Pat Williams) is instructive on these issues, as is the bold if controversial appropriation of the analysis of "infrapolitics" that Robin Kelley has applied to historical study of black politics in his most recent work. The analysis of the what happens to black identity as gender and class among other identities have come to the fore is a critical issue for political scientists to embrace, and some are; but this is a focus missing from this volume.

This problem is linked to two others. The first, is that one could wish for a more multidisciplinary approach to the study of black politics. The strength of African American Studies research has often been its multidisciplinary orientation. Indeed, Crenshaw and Williams are legal scholars, and Kelley is a historian. Elsa Barkley Brown has written extensively and eloquently as have other historians such as Brooks-Higginbotham, Saville, and Lewis about the divisions present in nineteenth- and early twentieth-century black community politics. The theoretical frameworks adopted by these scholars are often, not always, different from those adopted by most political scientists. Indeed, black feminist theorists and theory have made stunning contributions to our understanding of the interaction between contested identities and politics. The section on black gender politics would have been further strengthened if more scholars from this perspective would have been represented in the volume. The exclusion of black women from black community politics has been as consistent a theme as the exclusion of blacks from American politics. The section on gender politics is especially useful as the work of authors such as Harmon-Martin indicates directions that black political research should proceed due to the special circumstances that black women face when participating within the political arena.

In conclusion, this volume continually reinforces the view that much of what has constituted the black political experience is dramatically different. The book also demonstrates that enormous amount of basic research into the contours of the history of this experience needs to be done. This research will yield enormous benefits to students of American politics. This volume represents a large step in that direction. True, the works that are represented in this volume have at times a somewhat too narrow point of view. As I argued earlier, the volume would benefit from a somewhat wider disciplinary and theoretical cast, as well as from a more nuanced view of how quantitative and qualitative studies should complement each other in the task of theory building. The bottom line, however, is that this is a volume that belongs on the shelf of any student of American politics, or as important, those of us who wish to understand the roots and contours of this country's most divisive and enduring social conflict.

University of Chicago MICHAEL DAWSON

COMPARATIVE POLITICS

Revolutionary Discourse in Mao's Republic. By David E. Apter and Tony Saich. Cambridge: Harvard University Press, 1994. 403p. $34.95.

This book is an analytic narrative à la Foucault of the ideological redirection, confirmation, and consolidation of the Chinese Communist party brought about by Mao Zedong in Yan'an from 1937–45. The flavor of the book is best conveyed in its own words: "Indeed, in all major revolutions a certain reciprocity will obtain between violent actions choreographed within master narratives and master narratives realized in the form of essential texts. . . . Body acts become anchored in language acts. Then, in the peculiar alchemy of transcription, violent events become experiences, experiences become sequences, estrangements and intimacies become the marks of punctuation, and a surplus of signifiers becomes embodied in a logic of meaning" (p. ix).

Revolutionary Discourse is a substantial work, the product of two eminent political scientists (one a senior China expert) involving multiyear collaboration, as well as a considerable number of interviews with Yan'an veterans. Nevertheless, it tells its story with the complacent subjectivity of an armchair essayist, not deigning to break cadence to attend to the quibbling reader's concerns for explicit argument, definition of key terms, or other matters that might imply an objective context beyond the vivid virtuality of the narrative.

So this is a thing of few bones and much supple tissue. An x-ray of the argument hardly does it justice, and the flavor of the book would be lost if its own vocabulary were not used. The basic thesis is that the key event of the revolutionary mission of the Chinese Communist party was Mao's establishment of a "discourse community" in Yan'an in the early 1940s. This involved a reinvention of Chinese history, of the Party, and of Mao himself in a set of nested narratives. As he reinvents himself as the Odysseus of revolutionary history, Mao becomes the Socrates of the new discourse community. In the "revolutionary simulacrum" of Yan'an, these new narratives become the core of a new Republic of the Caves by means of "exegetical bonding" through the community study of new sacred texts and collective self-transformation of individualistic perspectives. The new utopic (or "utropic") discourse community thereby became "logocentric," rather than "econocentric" and gave direction and impetus to the next 60 years of Chinese history.

Evaluating this book is so easy that it is virtually impossible. It is written throughout with a linguistic self-consciousness that constantly challenges the reader. Some readers will be titillated by Mao's first appearance in a new intellectual medium; others will find it intolerably frustrating to read Mao by the oblique and dappled light of postmodernism. For both groups, the decision will be based on the immediate relationship of the book to the reader's linguistic energy level and willingness to be told the tale, and the decision will be irreversible. Some parts of the book might be prised from the general narrative and assayed independently, but fewer than one might expect. There is extensive and detailed discussion of education in Yan'an and some brief focus on the oral histories collected, but even the oral histories are mostly blended into the general story.

As a work on Chinese politics, *Revolutionary Discourse* has the merit of emphasizing the importance, for the Chinese revolution, of intellectual and moral solidarity. The revolution was led by young intellectuals in the hills, rather than by green-shaded utility-maximizers; and it is an important question how they all came to march in the same direction. Moreover, the authors manage to highlight intellectual issues without getting bogged down in ideological detail.

On the other hand, for a book emphasizing texts and discourse ("writualization"), it is remarkably cavalier in its treatment of Mao's own writings. Mao's major philosophical texts of 1937 receive no more attention than this: "After a none-too-successful diversion into lectures on dialectics in the winter of 1936–37 . . ." (p. 59). This single clause disposes of the Yan'an writings that Mao considered his most important and that have been described by other analysts as central statements of Mao's thought. Moreover, the authors pay little serious attention to Mao's intellectual and political development before the Long March, and their own narratives of Mao's four political struggles are told with a grand self-confidence that rivals that of Mao's stories. Different stories could be told—and these four stories could be told much differently—even with the same evidence. *Revolutionary Discourse* is not Mao's story, and it is not primarily an analysis of Mao's story. It is an informed tale about Mao and Yan'an.

It is interesting to imagine how this book would have been received by Mao and his colleagues in Yan'an, especially considering Mao's considerable repertoire of earthy expletives. Of course, acceptability to the original participants is not a test of validity, but it reminds us that such a narrative has no more privileged a relationship to its subject than a statistical or a rational actor approach.

University of Virginia BRANTLY WOMACK

The Long Twentieth Century: Money, Power, and the Origins of Our Times. By Giovanni Arrighi. New York: Verso, 1994. 400p. $21.95.

The world system resarch program is now about 20 years old. In that time, several different perspectives have emerged and undergone elaboration and (in some respects) cross-fertilization; that is to say, the theories have become richer, more explicit, and somewhat more overlapping in spirit, if not in vocabulary. They have also become temporally more ambitious. The initial focus was aimed at the last five hundred years of change. Some analyses have pushed this benchmark back several hundred years, while others prefer to go back even further to encompass the last five thousand years or more. Here the emphasis is on significant continuities in structure and process that permit one to make generalizations across really long stretches of time. Another thrust has been toward more empirical testing of these generalizations. The testing efforts leave much room for improvement, but it can be said that the empirical foundation on which world systems analysis rests is becoming more, rather than less, supportive.

One of the most recent contributions to this literature is the misleadingly titled book by Giovanni Arrighi. Very much to Arrighi's credit, the book is not only about the "long twentieth century," which for Arrighi means the period from the 1870s into some undefined endpoint in the twenty-first century. Probably more than half the book is devoted to theoretical development and to analysis of the three "long centuries" that preceded the one in which we find ourselves currently: 1340s–1630s, 1560s–1780s, and 1740s–1930s. So what this book is really about is Arrighi's interpretation of the modern world system over the past 650 years or so.

Arrighi offers a novel and synthetic repackaging of a number of insights from an extremely diverse crew, ranging from Adam Smith, Karl Marx, and Henri Pirenne to Ferdinand Braudel and a large number of prominent analysts of the late twentieth century. By no means is the term *synthetic repackaging* used disparagingly. Arrighi makes an effort to integrate within his perspective ideas and arguments that he finds useful in deciphering fundamental political-economic processes. He explicitly acknowledges wanting to pick up where Braudel left off chronologically and theoretically. He does that and more. The resulting synthesis amounts to a major new slant on what makes the world tick, and yet it is one that will frequently sound familiar because it combines a number of arguments and observations that have been around for some time.

The heart of his interpretation centers on *three segments in each successive long century*. The first and third segments revolve around financial expansion. In the first segment, the leading agencies of capital accumulation begin switching their capital resources away from trade and into financial speculation and intermediation. They do this because they perceive diminishing returns from further investment in established trade. Specializing in financial activities offers an opportunity to prolong leadership/dominance and a temporary renewal of profitable returns.

At the same time that the old regime is on its last legs, the next regime, built around specific governmental and business institutions and strategies, is emerging in a new region of the world economy and under the guidance of new leading agencies. Actually, Arrighi suggests—intriguingly but without sufficient elaboration—that more than one new potential regime emerges but that only one emerges triumphant to reorganize the world economy. The full development of the new regime takes place in the second segment, which focuses on the material expansion of the world economy. When profits start to lag, the third segment begins as the capital switch from trade to finance occurs again and still another new accumulation regime begins its rise to preeminence away from the center of the last regime.

In Arrighi's script, we have gone through this three-phase movement not quite four times. The first time extended from the 1340s to the 1630s under Genoese leadership. The second round, 1560s to 1780s, was Dutch-led. The third iteration, 1740s to 1930s, was British. The fourth one, led by U.S. firms, began in the 1870s and is not yet quite over. Arrighi avoids closure on the question of whether a fifth accumulation regime, centered in Japan and East Asia, is emerging; but he seems to favor this position as something more than one of several equally plausible possibilities.

In developing his framework, Arrighi makes several contributions in several ways that do not hinge on whether one accepts, or balks at accepting, his point of view. One clear contribution is highlighting the process of shifts from capital investment in trade and production to finance. This is an area that we all recognize as important but somehow have not quite found time to focus upon. Rather than make it an ancillary question, Arrighi builds his entire historical-structural edifice around the movement back and forth between trade and finance.

A second type of contribution concerns distinctions made about how different regimes are different. Arrighi argues that earlier historical-structural interpretations have given the impression that leadership structures are invariant; that is, the system cycles from one structure to the next without undergoing real change. Arrighi hypothesizes that there is, instead, a cycle within the leadership cycle with "cosmopolitan-imperial" regimes (Genoese, Britain) giving way to "corporate-national" regimes (Dutch, United States). The former specialize in widening the geographical parameters of the world economy while the latter emphasize deepening and consolidating the economic processes.

While there is much to recommend that this book deserves a wide readership, inevitably, there are several areas that deserve more attention than they receive. One is a matter of style. Arrighi writes in a relatively traditional and European interpretative style. It is a style that is easy and even a pleasure to read but the reader is forced to sift the theoretical arguments from the descriptive observations. Very precise statements (e.g., the timing of the long-century segments), are left to a single figure buried in the back of the book with the rest of the book's figures. Arrighi may have improved on Braudel's arguments, but now somebody else needs to formalize Arrighi's theoretical statements and hypotheses so that we can better compare them to others for testing purposes.

A second area that deserves more attention, ironically, stems from one of Arrighi's strengths. Most analysts stress the material foundations of rise-and-fall dynamics and slight the role of finance. Arrighi reverses this problem. Not only is he more interested in the role of financial intermediation, he frequently gives the impression that it is the regime "software" (its organizations and institutions) that are most responsible for the emergence of new accumulation regimes. At times, Arrighi seems to recognize that technological and commercial innovations (the "hardware" of economic change) have some role to play, as well; but they are omitted from theoretical scrutiny. If, on the other hand, it is the maturation of radical innovations that leads to the fall in profits that is one of the drivers of Arrighi's political-economic system, Arrighi's theoretical edifice must then be seen as significantly underspecified. Somehow, we need to integrate technology, institutions, strategies, and finance.

An abiding concern of historical-structural analyses, especially in political science circles, is the light that can be shed on patterns of war and conflict. Arrighi is disappointing on this dimension. Essentially, he falls back on the Hobson-Lenin imperialism argument without really exploring its degree of fit to successive bouts of intensive warfare. Yet he also suggests an interesting hypothesis about the rough synchronicity of increases in the supply of money capital and the demand for it by agencies primarily interested in the conquest of territories, markets, and populations. This is another hypothesis that deserves greater empirical attention.

Finally, Arrighi's pushing back his temporal boundaries to the 1340s is welcome even if his treatment of Venice and Genoa are among the more debatable aspects of the enterprise. But—in another book perhaps—it would be interesting to see how he views this long-century structure and process emerging in the fourteenth century. Where did it come from? Why did it emerge in the fourteenth century? Why did it emerge in the Mediterranean region? Do the circumstances surrounding its emergence give any clues to a possible movement away from the long century's structures and processes?

Nevertheless, *The Long Twentieth Century* is an excellent book. Whether one agrees with its arguments and assessments or not (and my own copy is studded with question marks in the margins), there is much to be gained from studying its theoretical arguments and historical applications. Let us hope as well that Arrighi has more to say on "money, power and the origins of our times."

Indiana University WILLIAM R. THOMPSON

Burying Mao: Chinese Politics in the Age of Deng Xiaoping. By Richard Baum. Princeton: Princeton University Press, 1994. 489p. $34.95.

Chinese politics often seem irrational if not inscrutable. Much of the work of specialists on Chinese domestic politics seeks to uncover principles that will clarify the seeming disorder of Chinese political life. Trained in American social science, we believe that we can uncover frameworks and rationalities that will unlock the mystery that is China. During Mao's lifetime, China specialists learned to read between the lines of Mao thought and to interpret clothing styles and photos on the front page of *Renmin Ribao*. Since Mao's death, economics and sociological insights have helped to broaden the intellectual basis for deciphering political events in China, but there clearly are limits to their efficacy as tools for understanding leadership conflict in China. What has been needed is an approach that can combine the insights of modern social science research with the interpretive tools of rhetorical analysis.

Richard Baum provides just such an approach. His lavishly documented narrative of the lurch from crisis to crisis since Mao Zedong's death in 1976 is a blow-by-blow account of the factional infighting and policy ramifications of politics since Mao was buried. His interest, of course, is more than just telling a political story. What Baum wants to do is to provide an analytical interpretation of the remarkable policy and ideological swings we have witnessed since economic reforms began in China in 1978.

While undoubtedly overly detailed for both the mass public and nonspecialists in political science, Baum performs a major service for those interested in the ebbs and flows of recent Chinese politics. Here we can read a much fuller story of the events leading up to the arrival of tanks at Tiananmen in June 1989. Baum is careful to report the official line on the ultimate events of the Beijing Spring, as well as the consequences of the split within the student movement, the levels of support from reformers in the Chinese Communist party (CCP) and among intellectuals and workers' movements. A similarly insightful and useful analysis is provided for the predecessor to Beijing Spring—the student movements in the winter of 1986/87. This is the most complete and detailed account I have read of the movement: from Fang Lizhi's speeches to Hu Yaobang's apparent support, from student activism in universities to conservatives' horror at the chaos in the streets. Here, too, we can learn about the responses and reactions of various high-level leaders within the CCP and the tensions among the factions that were heightened by that movement and the subsequent attack on bourgeois liberalization within the Party itself.

Baum's aim is not just to provide an exciting and complete narrative. Rather he offers here an interpretation of the *fang-shou* (roughly, "letting go–tightening up") cycles in China, putting Deng Xiaoping at the center of the fulcrum in the movement between *fang* and *shou*. Economic policy, although clearly a success at the macro level of economic growth, has been subject to fits and starts, to backpedaling and fears of capitalism. So, too, an apparent willingness to allow China's citizens to talk about democracy has been consistently followed by campaigns to protect China from individualism and pluralism. This cycle of reform and retrenchment is often understood as a consequence of factional battles within an elite that may want to give the appearance of collective leadership but often fails to reach consensus. As other China scholars have recognized, Deng has been able to maneuver among members of the Old Guard, as well as to make use of the factional politics that are a structural feature of CCP life. Baum's contribution is to recognize the complex nature of the *fang-shou* flux and to pinpoint Deng's role as the pivotal motive force providing coherence as policy and politics move back and forth.

Baum does not provide a simple interpretation. He notes that there are at least five forces that interact to produce shifts in policy and ideology: the turbulence produced by irregular and asynchronous policy fluxes, exogenous factors such as the Solidarnosc movement in Poland or the disintegration of the CPSU, leadership crises, intergenerational change, and shifting factional alignments. Baum demonstrates persuasively (using a wide variety of Chinese sources, as well as interviews and secondary materials) that in every major event in China since 1978—from the democracy movements to the economic reforms, from the attacks on Western liberalization and bourgeois democracy to the mobilization of armed troops against civilians—Deng Xiaoping has been "at the epicenter, . . . personifying its manifold political convolutions and complexities" (p. 16). Despite retirement, Deng remained the central figure in shaping the survival of the regime. Now, after the death of Deng's rival and colleague, Chen Yun, the importance of Deng is further underlined.

As Baum convincingly argues, Deng Xiaoping has been able to bury Mao Zedong and his revolution. In its place is a radically changed economic environment, guided by a different (and insecure) interpretation of the meaning and importance of Marxism. Baum does not predict the future after Deng is gone. Will he, too, be buried? Has Deng been able to create permanent and routinized institutional structures that do not require his persona to confer coherence? The economic reforms are irreversible; but where are the political structures that can shape, control, and funnel conflict? Baum does not answer fully this question; but a reading of his narrative does suggest that at least in this sense, Deng has failed. Without Deng, it is not clear what or who will be able to resolve the internal dissonance and division with the Chinese political elite.

Indiania University JEAN C. ROBINSON

African Capitalists in African Development. Edited by Bruce J. Berman and Colin Leys. Boulder: Lynne Rienner, 1994. 275p. $45.00.

In the search for causes of the economic crisis in Africa, much has been written about the shortcomings of capitalist classes. The literature on modern entrepreneurship portrayed African business elites as having low achievement motivation and favoring trade over production. The dependency literature saw Africa's capitalist classes as either agents of foreign capital or products of bureaucratic privilege. Because incipient middle classes in Africa have tended to consume, rather than invest, economic surpluses, they have generally been regarded as ineffective agents of capitalist development.

In updating the record on African capitalists, this book makes valuable empirical and theoretical contributions. It helps delineate a postdependency perspective on the origins and economic consequences of social class formation by introducing considerations of political agency, class autonomy, and the roles of the state and market in promoting capitalism. The editors associate themselves with an emerging school of scholarship—first outlined by John Iliffe's *Emergence of African Capitalism* (1983) and Paul Kennedy's *African Capitalism* (1988)—that makes room for capitalist development within Africa. At their most wishful, the editors even consider that African development experiences might point the way toward an alternative framework to "the paradigm of modernity" that suffuses both Marxist and Weberian thought (see p. 239).

The volume is composed of a set of seven country case studies and three "think-pieces" based on comparative materials, all derived from a workshop on African bourgeoisies held at Queen's University, Ontario. The contributions are linked by common questions: Does development require a national bourgeoisie? What are its characteristic functions? Under what conditions has such a class emerged in African countries?

To my reading, there are five important findings. First, homegrown investment is occurring within African countries, capitalist classes are forming, and "indigenous capitalism of some sort is . . . in the process of development" (p. 190). Among the countries studied, these processes have flowered most fully in Cote d'Ivoire, where a nontraditional class of entrepreneurs, originating in plantation agriculture but now rooted in a range of secondary enterprises, has become ideologically self-conscious and politically organized (chap. 3). To be sure, the size and capacity of the local capitalist class varies considerably across African countries, sometimes being limited to "regional clusters of indigenous firms operat[ing] on capitalist principles," as in the eastern Nigerian enclave of Nnewi (p. 158).

Second, the formation of classes involves not only the acquisition of technical and economic skills, but the mobilization of cultural and political resources. In Africa, capitalists emerge around sets of personal, familial, and ethnic ties, some of which penetrate the apparatus of the state itself. Class interests are promoted by means of hometown and old-boys' networks, some of whose members hold top decision-making political offices within government. In the West, one historic mission of the bourgeoisie was to disseminate a hegemonic culture of instrumental rationality; in Africa, emergent bourgeoisies mix universal secular norms of modernity with local social values. To the extent that African capitalists deploy cultural idioms, they advance their interests at the risk of inadvertently impairing economic efficiency.

Third, accumulation of capital in Africa does not occur primarily in large-scale, high-technology industries. Instead, case studies in this volume point to the proliferation of small- and medium-scale enterprises—mostly in the informal sector—that undertake industrial production in forms more appropriate to Africa's present level of development. For example, in Kenya, microenterprises in the *jua kali* (outdoor) sector have expanded rapidly, won subcontracts from formal manufacturing firms, and gained official recognition from policymakers (p. 83). By heightening social trust and reducing information costs, small enterprises play "an important transitional role as national income increases and production shifts from agriculture into manufacturing" (141).

Fourth, Africa's capitalists may be immigrant groups (e.g., Indians in Kenya, Europeans in Zimbabwe), who, though not indigenously African, have become long-settled nationals. These groups commonly lead the formation of political groups that cut across cultural identities to promote class-based economic interests. This volume corrects earlier misconceptualizations in the literature that analysed these elements as foreign, rather than domestic, capital.

Finally and most important, the authors conclude that an effective and efficient state is essential to promoting capitalist development in Africa. Unlike in Asian newly industrialized countries, where strong states provided infrastructural support and economic incentives for private investment, African political leaders have used the state machinery to prey upon centers of accumulation in society. Because incumbent officeholders follow a political logic of reproducing privileged access to rents, they have usually sought to thwart the formation a class of potential capitalist rivals (see Boone on Senegal and Nicholas on Zimbabwe). Contrary to the neoliberal orthodoxy represented by economic adjustment strategies of the World Bank, the editors of the present volume regard the construction of developmental state (promoting, e.g., small-scale industrialization) as the way ahead.

For an edited volume, *African Capitalists* is tightly focussed on a single theme, blurring only as different authors treat social classes as a dependent or determinant variable. While some contributors seek to reveal the cultural and political conditions that favor the emergence of capitalist classes, others are more interested in probing the effectiveness of such classes at accumulation. The incisive chapter by Lubeck and Watts on Nigeria best attains the editors' goal of establishing a historical chain of explanation linking structural preconditions—through class formation—to economic performance.

And debates among the contributors raise as many questions as answers. Is the second economy a temporary pathology or a foundation for accumulation? Do personal ties help or hinder the emergence of instrumental rationality? Will independent centers of accumulation be nurtured by state policy or attract the unwelcome attentions of a predatory state? One detects a major tension—even disjuncture—between the editors' pessimistic answers to these questions and the more sanguine analyses of the country case contributors. Berman clearly concludes that Western forms of capitalism are not reproducible in Africa, and Leys caustically comments, "To obtain a class of parasitic nouveaux riches is one thing, but a class of serious wealth creators is quite another" (p. 12). Perhaps mixed signals are inevitable from a continent whose country development trajectories—from the promising industrial democracy of South Africa to the collapsing political economies of Liberia, Somalia and Rwanda—are diverging in multiple directions.

Oddly for a 1994 publication by leading scholars of African politics, the book makes only passing reference to the prodemocracy struggles that have engulfed Africa since 1990. This lacuna is notable, given the fact that capitalist entrepreneurs have become prominent leaders in opposition parties and elected governments in Africa's new era of multiparty competition. Rapley comments, "Prodemocracy movement(s) have not been an assault on bourgeois power; instead the changing nature of the capitalist class is leading to demands for—or at least a willingness to accommodate—new political structures" (p. 51). More analysis along these lines would have been welcome.

Michigan State University MICHAEL BRATTON

Controlling Immigration: A Global Perspective. Edited by Wayne A. Cornelius, Philip L. Martin, and James F. Hollifield. Stanford: Stanford University Press, 1995. $49.50 cloth, $17.95 paper.

In recent years, immigration has become a central issue of politics and public policy in the advanced industrialized states. The last decade has been characterized by accelerated migration movements, a widening of the range of nationalities and regions of immigrants, and a diversified means of entry used by migrants, particularly requests for asylum. Politicians and the media frequently speak of "loss of control" when they publicly discuss immigration. In the United States, persistent illegal immigration along the U.S.-Mexican border, fear of increasing terrorist attacks, and mass refugee influxes from Cuba and Haiti have intensified public perception that the government has lost control of its borders. After many years of relying on foreign guest workers to spur their economic growth, many European governments are now confronted with the unwanted task of assimilating large numbers of culturally different, permanent resident aliens and their offspring. In Japan, the absorption of large numbers of foreign workers into a racially and culturally homogeneous society is rapidly emerging as a major political and social issue. In response to these developments, all industrialized states have adopted broadly similar policies for controlling flows by systematically increasing their immigration controls and tightening their entry requirements. However, these measures do not seem to work, and most government officials do not believe that they can regulate immigration flows through control mechanisms alone.

Controlling Immigration: A Global Perspective is an impressive collection of essays by an interdisciplinary research team of immigration specialists. The objective of this book is to explain why the "gap between the goals of national immigration policy (laws, regulations, executive actions, etc.) and the actual results of policies in this area (policy outcomes) is wide and growing wider in all major industrialized democracies." Comparing immigration policies and policy outcomes in nine industrialized states (the United States, Canada, Britain, France, Germany, Belgium, Italy, Spain, and Japan), the authors explain both why certain immigration control measures have been adopted and why these measures have usually failed to achieve their stated objectives. The central thesis of this volume is that the difficulties of immigration control in the industrialized states today are closely linked to the rise of rights-based politics in postwar Western Europe and North America. This is particularly evident in debates over immigration, naturalization, and asylum policies in France, Germany, and the United States. Expanded rights for aliens in Western governments both help immigrants to gain admission under liberal asylum procedures and enables many immigrants to remain and even eventually to bring family members. Thus, despite increased pressure by antiimmigrant U.S. public opinion on their government to reduce immigration in recent decades, the annual ceilings on legal immigration have been increased and only weak measures have been adopted to prevent illegal immigration.

Several of the book's authors are skeptical whether the current array of human and civil rights for noncitizens in most industrialized states can be maintained, particularly in the face of rising domestic opposition to the current influx of foreigners and the collapse of some longstanding rights-based political coalitions. Immigration and refugee policies are becoming progressively more restrictionist in practically all Western states. Examples of growing restrictionism include the U.S. policy of interdicting Haitian, Cuban, and Chinese refugees on the high seas in order to prevent them from reaching U.S. territory and gaining access to the refugee admissions process; the German decision radically to tighten the provisions of its asylum law, which used to be the most liberal in the West; and new powers granted to French police to carry out random identity checks on foreigners.

The editors also challenge, and seek to clarify, several theoretical explanations of international migration in the modern world. According to economic liberalism, individual economic incentives drive international migration, while Marxist analyses suggest that international migration is determined by structural inequality in the global economic system. While both of these perspectives offer a plausible explanation of international labor migration, the editors maintain that politics, as well as economics, is important in shaping international migration flows. In particular, earlier analyses of international migration do not clearly explain how liberal norms have constrained state efforts to regulate international migration. For example, relatively generous immigration outcomes in the United States can be traced to norms advanced by a coalition of liberal interest groups, including civil rights groups, churches, employer associations, and ethnic associations, which until recently have dominated the debate over U.S. immigration policy.

Although this study focuses on the domestic sources of immigration policy, it is important also to emphasize the role of foreign policy, national security concerns, and international relations in the setting of the migration policies of the advanced industrialized states. In the United States, for example, foreign policy concerns strengthened proimmigration forces throughout the Cold War. Bipolar conflict with the Soviet Union resulted in calls for the admission of immigrants and refugees from communist countries and the liberalization of entry requirements for those "voting with their feet" in the ideological struggles of the period. For the United States, the influx of refugees and migrants, especially from adversary countries, made a powerful symbolic statement about American foreign policy goals and its vision of its place in the international system. For example, the arrival in Florida before 1980 of one-tenth of Cuba's population—and many hundreds of thousands since—has been viewed as revealing the bankruptcy of the promises of communism and the superiority of the free-enterprise system. A national sense of obligation or guilt because of past political and military involvement can also affect a nation's immigration and refugee policy, such as the open-armed welcome the United States gave to Indochinese refugees for over a decade. International political commitments, largely rooted in past imperialist ties, have influenced the immigration actions of several Western European governments. In the two decades after World War II, France, Great Britain, Portugal, and the Netherlands admitted overseas subjects ranging from Algerians to Vietnamese to Jamaicans to Indians to Timorese to Moluccans from the Dutch East Indies because of strategic and foreign policy links with their former colonies. Similarly, historical bilateral linkages and state responsibility for the fate of former subjects are likely to influence Britain's response to any outflow from Hong Kong after 1997. Finally, it is increasingly important to understand the close linkages between ethnic groups, their home countries, and foreign policy factors. Migrants often become a political force in their host country, influencing its policies and particularly its relationship with the country of origin. These are but a few of the many examples one could give of the ways in which the immigration policy behavior of states is affected by international relations.

This is a fine book. Its comparative analysis of the domestic sources of immigration policy contributes greatly to our understanding of an extremely contentious policy problem.

University of Notre Dame GIL LOESCHER

Back from the Future: Cuba under Castro. By Susan Eva Eckstein. Princeton: Princeton University Press, 1994. 286p. $29.95 cloth, $15.95 paper.

This book about the accomplishments and current crisis of the celebrated Cuban model of development comes very timely now that the demise of the final remains of communism and centrally planned systems seems to be a matter of time only. What are the odds that the Cuban brand of socialism would stand a chance to survive in the midst of the apparent triumph of neoliberalism in the post–Cold War era?

Many questions arise. Is there any uniqueness about the Cuban model that would make it less predictable (to collapse) than has been the case in the Soviet Union and Eastern Europe. One of the many jokes that circulated in Eastern Europe in 1989, just prior to the final collapse, was the following answer to the question how to define socialism: "It is the long, cumbersome road from capitalism to—capitalism" Eckstein gives in *Back from the Future* a convincing account of how Castro's Cuba (or perhaps, rather, Castro himself) has gradually, although reluctantly, retreated from a push for the communist utopia in the 1960s to a more realistic socialist goal in the 1970s and 1980s, just to have to cope with the very survival of the regime in the 1990s.

Eckstein claims, like many other observers of Cuba, that the Cuban Revolution is unique and that that is why its final fate cannot be judged by looking at the fate of the Soviet Union and Eastern Europe alone. Even during the Rectification Process in the late 1980s, there was a surprising flexibility in the Cuban strategy, with an apparent return to a more orthodox Marxism-Leninist model—or, rather, its Cuban variant inspired by the socialist morals set by the legendary Che Guevara in the 1960s. This new policy was not only inspired by "back to the basics," but it was also coupled with the promotion of foreign investment and more leeway and independence for state enterprises.

The book gives an excellent overview of the economic, social, and political development of the revolution. The architect behind all, both in the "push for communism" and in the "retreat to socialism" is, of course, Fidel Castro himself. His charisma and undisputed leadership has no doubt played a leading role in the destiny of Cuba during the last 35 years, although I agree with Eckstein's hesitation to qualify him as just another benevolent dictator. As has been mentioned by many other authors, as well, there are many seeds of democratic institutions in revolutionary Cuba, even if Cuba, of course, is far from being a democracy in the Western sense.

One of the most interesting theses in Eckstein's book is what she terms "the irony of success," meaning the "demographic revolution" that has been the result of the social reforms that started just after the triumph of the revolution in 1959. Social reforms are, as a rule, very expensive, especially for poor countries. There is no free lunch, and such reforms have to be financed either through the state budget, or through exports or foreign assistence. In the Cuban case, it is quite clear that had it not been for very beneficial terms of trade with the Soviet Union during critical years, there would not have been funds for the very ambitious social reforms.

But it is just not that social reforms are expensive. In the Cuban case there has been a "demographic revolution," says Eckstein, primarily as a result of the success of the reforms. With fertility rates going down (although partly offset by declining infant mortality rates) and comprehensive social security and health care schemes for the older, there has been a gradual shift in the age pyramid. The more successful the reforms, the more burden is put on the state's finding the ways and means of financing this growing social reform bill. This is, of course, nothing new for many welfare states in Europe, which are going through the same kind of crisis these days. Sweden is a case in point. The challenge for Cuba is, of course, far greater, with an economy in bad shape after the collapse of the Soviet Union.

A problem Cuba is facing today is the very credibility of the revolution. If the Cubans follow Western advice (i.e., following the Russian road back to some kind of capitalism), it is very likely that there will be very few social achievements left to be proud of. If the social achievements are to remain, in justification of the necessity of the revolution, they have to be financed somehow. This financing has to come from some new export outlets and profound economic restructuring. A lot of new thinking is taking place these days in Havana. Eckstein's book was finished in 1993 and cannot, of course, deal with the number of economic reforms that have taken place in Cuba since; but even so, it is surprising that Eckstein does not devote more space to a discussion on various alternatives, or scenarios, for the Cuban Revolution.

It is clear that Eva Eckstein has great sympathies for many aspects of the Cuban Revolution; but instead of presenting and discussing various alternatives of how to cope with the present crisis, adapting to a "new world order" while at the same time safeguarding the social achievements, she seems to prefer just to wish it good luck. What is also surprising is that Eckstein devotes so little space to a discussion of the impact of the U.S. embargo against the island, the prospects for lifting it, and when it is finally lifted, what prospects and consequences for U.S. business and for the Cuban economy may be expected.

These shortcomings should, however, not hide the fact that Eva Eckstein's book is an impressive study about the achievements and subsequent crisis of Castro's Cuba, and it is a welcome and useful contribution to our knowledge about the Cuban Revolution.

University of Lund (Sweden) CLAES BRUNDENIUS

Local Elites in Western Democracies: A Comparative Analysis of Urban Political Leaders in the United States, Sweden, and the Netherlands. By Samuel J. Eldersveld, Lars Stromberg, and Wim Derksen. Boulder: Westview, 1995. 273p. $65.00.

In *Local Elites in Western Democracies,* three distinguished political scientists provide a valuable and extremely well executed comparative study of local political elites in the United States, Sweden, and the Netherlands. Representing distinct political configurations (one federal, two unitary) and policy mixes (two generous welfare states, one austerely liberal), the three countries allow the researchers to consider the effects of such differences upon elite opinion. (The authors include a useful summary of central-local relations and local government in each country.) They selected 55 medium-sized cities from the three polities (20 American, 20 Dutch, and 15 Swedish) and conducted a questionnaire survey of the local political elite (technical details of which are included in an appendix). The questionnaire obtained information about both the socioeconomic profile of the elites and their attitudes to pressing fiscal and political problems. It was distributed to "positional elites" in the selected cities in each country, a category composed of both elected politicians and appointed administrators, though the balance of these two groups differed in each country: in the United States, 260 interviews were conducted (132 administrators, 128 politicians); in Sweden, 389 (128 administrators, 261 politicians); in the Netherlands, 305 (104 administrators, 201 politicians).

Eldersveld, Stromberg, and Derkson construe "elite political culture" as threefold: elite perceptions, elite evaluations, and elite values and preferences. This framework is consistent with standard conceptions of culture and political culture. Their primary question is, Why do elite political cultures differ in the three chosen cases? This problematic is closely related by the authors to extant comparative urban research and its salient propositions. Somewhat straining credulity, the authors reduce this work to a single issue: "If there is one overriding purpose or significance for our study, it is to describe and seek to understand for three modern democratic systems the local political elites which have emerged, or which have been recruited and selected, to deal with the 'urban political problem' in each of these societies" (p. 19).

What did these industrious scholars unearth? The concern (arguably, the obsession) with comparative differences fortunately does not overwhelm the interesting data collected here. The reader learns about the distinct educational, class, gender, and occupational background of the local elite in each country (finding, e.g., that women fare less well in local than in national politics in each country). Commenting on cross-national similarities, the authors observe: "Male dominance is obviously one. The extent of parental involvement in politics and government is a second. The considerable upward economic mobility of these elites, compared to fathers, is also apparent" (p. 52). In terms of perceptions, the research unearths a stronger sense of urban crisis among the local elite in Sweden and the Netherlands than in the United States (a finding that may explain why urban life in the latter is less convivial than in the former). However, as the authors acknowledge, the term *serious* may carry distinct cultural meanings, a somewhat egregious admission for a comparative study (p. 60). This problem prompts an anodyne conclusion from the authors: "Elite views seem to be often largely fashioned by and responsive to the particular economic conditions and political governance of the particular city in which these leaders have grown up. Our comparative theory must be built on that kind of empirical understanding" (p. 88). Of more consequence, the authors rightly draw attention to the pusillanimous failure of some local elites (notably those in the United States) fully to grasp the scale of local problems: "In the US there is low recognition that race, health, pollution, for example, are serious local problems; in Sweden it is health and immigration; in the Netherlands, despite the high average level of concerns for all problems, on health and education the proportions expressing concern are relatively low" (p. 89). The authors also bemoan the discrepancy—a pronounced one in each country—between local elites' acknowledgement of power and failure to address these problems. This conclusion, however, is somewhat at variance with a later one that a sizable proportion of local elites—particularly administrators—believe themselves to have little influence on policy, even though the same respondents also believe their cities already enjoy power appropriate to effect policy (pp. 152–53). In assessing for political values, Eldersveld, Stromberg, and Derkson find that local political elites are localist and strongly supportive of democracy. Finally, from their survey replies, they offer some conclusions for the recent debates about "state versus market" processes as determinative of urban political outcomes: "Along with the economic status of the community and its political institutions such as the party system and pressure group complex, our findings indicate that the type of political leadership, in its broadest meaning, must be considered as playing a central role" (p. 229). This conclusion implies the continued utility of those approaches to urban politics which stress the role of the state and politics.

Overall, this is a pithy and lucidly written book, not overburdened by the extravagant use of social science terms, which provides a well-researched profile of local political elites in three democracies. The authors are perhaps overambitious in their commitment to "general laws," and their aspiration to improve urban conditions is overstated (tendentiously captured on p. 250 as "These are not phantom cities! These are live data and live cities!"). However, the authors expatiate sensibly upon their findings, and these latter will be of considerable value to other researchers in the field.

University of Oxford DESMOND KING

Democracy from Scratch: Opposition and Regime in the New Russian Revolution. By M. Steven Fish. Princeton: Princeton University Press, 1995. 300p. $39.50.

From its unorthodox title to the bold conceptual innovations, Fish's book rejects many established views concerning the end of Sovietism and the rise of the new Russian state. Unlike (for instance) Moshe Lewin in *The Gorbachev Phenomenon* (1990), Fish does not believe that a genuine civil society had emerged in Russia before Gorbachev's rise to power. The reason for this was the tentacular, monopolistic, and expansionist nature of the state and the intensity of its extractive (parasitical, one could say) attitude toward society (more persistent and systematic in the Soviet case than in Poland or Hungary, where networks of civic opposition could emerge and develop).

For Fish, Western studies of Soviet affairs (widely described as Sovietology) have failed to take into account the total nature of the Leninist regime and its all-embracing impact on the various social subsystems. The decline of ideological coercion and conformity is seen—in the tradition of the Budapest School represented by authors like Ferenc Feher, Agnes Heller, and Gyorgy Markus—as the major source of systemic degeneration and eventual collapse. Fish's approach is comparative and continuously relates events and ideas in Russia to trends in Central and Eastern Europe. With its clear emphasis on the theory of social movements and collective protest and the discussion on elite building and democratization, this book does a superb job in deparochializing Soviet and post-Soviet studies. But more importantly, Fish sheds new theoretical light on the dynamic of the transition from ideocratic authoritarianism. He refuses the interpretations based on political culture that tend to exaggerate the continuities in Russian history. By focusing on the state-society interaction, Fish points out the very possibility of the democratic revolution in a country often seen as deprived of traditions or memories of dialogue and tolerance. On the contrary, this study argues, democracy from scratch is possible as an exercise in democratic imagination. Tradition matters, of course, but not that much in the realm of values, opinions, and symbols, as in the one of institutions and norms.

Fish's book is impressive both conceptually and in terms of its substantive empirical data: the analyses of Russia's protoparties and the inner conflicts, tensions, and dilemmas within the new elites are well documented and insightful. In his view, a genuine civil society is characterized by strong, stable, and autonomous associations; and this was precisely the missing element in the late Gorbachev years. Instead, what emerged in that country, Fish argues, was a "movement society," in which the new associations and embryonic parties were as elusive, pesonality-dependent, and ephemeral as political campaigns. Fish finds the root of the absence of Western-style civil society in the enduring legacies of authoritarianism (Stalinist, pre-Stalinist, and post-Stalinist). Here, I would argue that he tends to underestimate the impact of dissident ideas in the 1960s and 1970s and their role in the articulation of alternative civic values.

In general, one can hardly disagree with Fish's diagnosis of the turbulent birth of Russia's democracy where groups and movements compete in voicing different, often irreconcilable visions of citizenship, distribution, and national self-definition. In this respect, Fish regards the role of ideological differentiation as a still valid criterion for distinguishing between parties and affiliations in Russia. Thus, one of the most salient features of this revolution in Russian politics are the difficulties associated with defining constellations of values and symbols able to energize collective loyalties. As Fish puts it, "To a far greater degree than in other contemporary transitions, the most basic elements of collective political identities had to be invented and assembled entirely from scratch" (p. 93). Indeed, the very nature of the pre-1991 regime—the weakness of the forms of political protest, the widespread conformity and resignation with the status quo—suggests similarities with the cases of Romania, Bulgaria, or Serbia, rather than Poland or the Czech Republic. In this respect, I think that Fish is right in noticing a "syndrome of ineffectiveness" as the crucial characteristic of this political stage (inchoate ideological allegiances, extreme personalization of the political discourse, absence of class-based political formations, prevalence of "passions" over "interests" in formulating political preferences and strategies). On the other hand, I think he could have insisted on the role of ethnocentric discourses and symbols in the constitution of the new (post-Bolshevik and postimperial) Russian identity.

Fish sees the Russian national revolution as the result of numerous local minirevolutions occurring more or less simultaneously but with different intensity and driven by different causes. Fish deftly explores the opposition movements in four Russian cities: Sverdlovsk, Tula, Volgograd, and Orel. He shows that in each of these cases, different events generated social mobilization and various types of responses on the part of the ruling groups. Fish's typology of the revolutionary political entrepreneurs during the transition period—the compromisers (often, but not always, opportunists), the pragmatic radicals, the saints, and the fanatics—is a unique contribution. This classification could be usefully applied to interpret many of the developments in East Central Europe following the revolutions of 1989.

Finally, Fish is convinced that democracy starts from scratch in Russia because of the uncompromisingly antidemocratic legacy of Leninism. Once this system has fallen apart, the new movements and parties have necessarily to construct themselves without any claim to a historical democratic pedigree (unlike, e.g., Poland, where the new parties could invoke their leaders' opposition to the communist regime). As for the movement organizations (the backbone of what Fish sees as the movement society), their raison d'être was to voice negation of the old order. In terms of positive impact, however, they have not succeeded in establishing a solid basis for interest representation. Fish remains moderately skeptical about Russia's future: instead of a consolidated democracy, he envisions a "democracy by default" that would include some elements of democracy, such as elections and civil and cultural freedoms but not durable and stable procedures, norms, and institutions based on genuine separation of powers (especially between the executive and the legislative). Another likely scenario would be a self-limited authoritarianism that would avoid the relapse into any form of ideological messianism and favor of a South Korean or Taiwanese model. Elegantly written, conceptually original, and intellectually provocative, Fish's book is a most illuminating contribution to the growing body of scholarship on political democratization and the problematic transitions from state socialism.

University of Maryland, VLADIMIR TISMANEANU
College Park

Marxism, China, and Development: Reflections on Theory and Reality. By A. James Gregor. New Brunswick: Transaction, 1995. 283p. $34.95.

This book consists of two parts that are counterposed to each other. The first part deals with deviations from a theory, Marxism, and their disastrous consequences in the form of economic and political failures in the former Soviet Union and China under Mao. The second part shows how conformity to a theory (i.e., the doctrines of Dr. Sun Yat-sen of modern China) reaps bountiful harvests—a "miracle economy" in Taiwan and its ensuing successful transition to democracy.

Gregor devotes more than half of the book to the first part. He reviews the essence of Marxism, the rise of Stalinism in the Soviet Russia, failure of the Stalinist economic system, the Chinese communists' copying Stalinism in China after 1949, failures of the Maoist economy, and economic reforms in post-Mao China. Gregor's main thesis is that the Stalinists and Maoists deviated from the Marxist stress on the primacy of productive forces. Instead, Stalin and Mao substituted "exceptional leaders" for productive forces and Party organization for the market mechanism. According to Gregor, the inefficiencies, wastes, and political despotism of Stalinism and Maoism stem from these ideological deviations. These are, so to speak, old territories that have been covered already by a number of scholars. Gregor does not blaze a new trail. His analysis smacks of both reductionism and economic determinism. Gregor attributes every socioeconomic problem in the former Soviet Union and Maoist China to absence of a market. He sees the origin of totalitarianism in the Stalinist and Maoist regimes' need to carry out "primitive socialist accumulation," which in turn is necessitated by national "economic desolation and industrial backwardness" (p. 266). These are not new insights either.

Gregor's discussions of Taiwan's economic success and political democratization are rather brief and clichéed. He recounts the usual superficial indicators of Taiwan's economic development and political change. There is no evidence of any new research on Taiwan by Gregor. His analysis is reductionistic and imbalanced. He thinks that the key to Taiwan's achievements is Sun Yat-sen's stresses on a market economy and linkage to the West. He sees Taiwan's transition to democracy as a fulfillment of Dr. Sun's teaching that after a period of political tutelage, the government ought to install Western-type democracy in China. But Gregor does not prove that Taiwan's development has been guided by Sun's theory. What he does is merely show similarities between Sun's teaching and the Taiwan's strategy for development. But Gregor does not demonstrate the actual causal mechanism and process linking Sun's theory with Taiwan's developmental planning. He obviously has not inquired with Taiwan's economic and political planners as to whether they were guided by Sun's teaching in the 1950s and 1960s when these leaders set Taiwan's main course of economic development. There is no discussion or even mentioning of these planners in Gregor's book. If he had taken the trouble to interview these Taiwanese leaders, Gregor would have discovered that they were directed primarily by practical concerns for Taiwan's economic survival, rather than by Sun's theory, which is too general and was intended for mainland China anyway. If there were any guiding human hands in Taiwan's economic planning, they belonged to the aid officials from the United States. Furthermore, Gregor does not mention the Kuomintang's many deviations from Sun's theory, especially in politics. His analysis is very skewed.

Gregor's presentation of Taiwan's democratization is seriously flawed. He describes the superficial signs of democracy, such as elections and new legislations to lessen political repression. He does not touch on the multiple causes of Taiwan's democratic transition, such as a fundamental transformation in the outlook of the new leaders of the Kuomintang (a possible feedback effect of Taiwan's successful economic development), rise of a militant opposition movement, and, most crucially, the crisis in Taiwan's international status since 1979 when the United States severed diplomatic relations with Taiwan.

In sum, the most fundamental deficiency in Gregor's book is his analytical inconsistency. In the first part of the book, Gregor's presentation is balanced, showing the Soviets and Chinese communists' conformity and deviations from Marxist teachings. But in the second part of the book, on Sun Yat-sen and Taiwan, Gregor stresses conformity (unproven) to theory and ignores deviations from it.

University of California, ALAN P. L. LIU
Santa Barbara

Civil Society and the State of Africa. Edited by John W. Harbeson, Donald Rothchild and Naomi Chazan. Boulder: Rienner, 1994. 311p. $45.00.

John Harbeson and his colleagues have put together a useful and comprehensive collection on a subject that has attracted wide attention from students of African politics. In 13 essays, 12 of which were culled from more than two dozen presented at a 1992 conference held at the Hebrew University, the authors explore the theoretical properties of civil society in both its classic and contemporary usage, as well as its many manifestations, in present-day Africa.

By the mid-1980s, most African states had become progressively repressive as the economies they overregulated unraveled and their capacity to govern declined. The societal response was a variety of coping strategies to ride-out hard economic times and obtain basic services that the state could no longer provide. At the level of the individual and household, urban and rural dwellers sought refuge in the informal economy or retreated into subsistence. At the level of the local community, residents periodically engaged in voluntary collective actions to provide themselves with police protection, water, education, and so on. These strategies by the poor were later complemented by demands for political liberalization and increased state accountability from more prosperous and organized constituencies, such as the church, urban-based professional associations, trade unions, and agricultural organizations. While the poor withdrew or partially withdrew from the state, the middle class engaged it.

As associational life emerged to overcome the decline of the state and official markets, the term *civil society* captured the imagination of both academic students of African politics and those seeking to nurture economic and political reform. For the former, *civil society* filled a theoretical void. Having explained African politics in terms of ethnicity, class, and/or dependency, many sought a new approach to understand the social forces underlying the changing relationship between the rulers and the ruled. For the reformists, *civil society* held out the Tocquevillian prospect of providing instruments of countervailing power that would force the African state to become accountable to the governed. One result, as Crawford Young notes in his chapter, has been an uncritical and "hopeful appropriation of the term in Africanist discourse. . . . Juxtaposed to a veritably satanic state was an angelic civil society" (p. 47).

With this orientation as their point of departure, the contributors probe the utility of the concept without reaching any consensus on its meaning. In an overly long yet valuable introduction, Harbeson argues that the prime function of civil society in contemporary Africa is to establish legitimate norms regarding the operation of the polity, that is to say, a new and viable constitutional order. Civil society is thus much more than the sum of associational life that exists between the household and the state and more specific in function and process than the totality of organized interests making claims on the state. By ascribing a normative function to civil society, Harbeson seeks to bridge contemporary Africanists' usage of the term with that of the European social contract theorists who invented it three centuries ago.

A series of three essays by Crawford Young, Michael Bratton, and Victor Azarya, grouped under the heading "Theoretical Perspectives," follow Harbeson's introduction. Young and Bratton present concise yet detailed reviews of the concept in the literature beginning with the European social contract theorists but reach very different conclusions as to its utility. For Young, civil society is burdened by "layers of received meanings." He questions whether it is truly an actor in the African context and concludes that it remains "an elusive concept and a more fugitive reality." He also calls attention to the downside impact of "exclusionary visions of civil society," as exemplified by Islamic fundamentalist parties seeking to impose their normative order on the polity. Bratton, by contrast, sets forth a five-point definition of the properties of civil society. Consistent with contemporary usage, he emphasizes that civil society exists in a dynamic relation to the state, a perspective also examined by Azarya.

Essays by Nelson Kasfir, E. Gyimah-Boadi, Aili Mari Tripp, Janet MacGaffey, Jennifer Widner, Jane Guyer, Thomas Callagy, Donald Rothchild, and Letitia Lawson and a conclusion by Harbeson round out the volume. These authors present a rich empirical analysis of how individuals and associations engage the state (and, in some instances withdraw from it) in Ghana, the Ivory Coast, Nigeria, Tanzania, Uganda, and Zaire. Included in their chapters are discussions of the activities of women's associations, farmers and farmer associations, trade unions, business and transportation organizations, professional associations, and so on. The likely impact of civil society on structural adjustment is also explored. The result is a volume that provides no definitive argument about the shape or significance of civil society but that fully elucidates this complex phenomenon.

University of Iowa JOEL D. BARKAN

The Velvet Glove: Paternalism and Conflict in Gender, Class, and Race Relations. By Mary R. Jackman. Berkeley: University of California Press, 1994. 441p. $38.00.

How do the wealthy few maintain their dominance in capitalist democracies over the working classes, despite obvious and enormous inequalities? How do men maintain their privileged status over women, or whites over racial minorities? The stability of such longstanding inequalities between major social groups has long been one of the core puzzles for social scientists.

Mary Jackman argues here that both dominant and subordinate groups share a great stake in long-term peace and harmony, overriding the latter's own self-interest. Dominant groups tend to seduce their subordinate counterparts with paternalism and rational persuasion. Hostility and conflict are bit players in these dramas, and open coercion and violence are utilized only as a last resort. They maintain control by persuading subordinate groups of an individualistic ideology that denies "the group basis of social life," thereby fragmenting collective resistance. Subordinate groups, in turn, therefore, only cautiously express their demands, rarely venturing into open conflict, hesitating to jeopardize the limited resources they already have.

To test these ideas empirically, Jackman, using a 1975 national survey, presents a "comparative case study" of three pairs of American groups yoked in long-term unequal relationships: whites and blacks, men and women, and upper and lower social strata (though this last proves to be a continuum, rather than a dichotomy). Both the theoretical context and Jackman's perceptive observations infuse even familiar findings with original insights. For example, she views segregation as a primary tool for sharpening status inequalities. Not surprisingly, white-black relations are marked by extreme spatial segregation, and male-female relationships, by much role segregation. But the extent of segregation is startling: men and women turn out not to have a lot of informal contact in the workplace or many close cross-sex friendships, either.

Most centrally, she takes on the conventional view that such intergroup relationships involve much hostility and overt conflict. Instead, she finds generally positive feelings across gender lines, and even race generates at most "muted hostility." Instead of widely perceived conflicts of interest between groups, she finds an "amicable consensus" among men and women that little conflict exists and that neither is harmed much by the asymmetries in their relationships. Even race and class relationships do not elicit a widespread perception of conflict. Nor do these supposedly contending groups hold sharply divergent policy goals. Instead, individualism turns out to be an overarching moral principle, "the hallmark cry of dominant groups under challenge in the capitalist-democracies" (p. 225). The great achievement of paternalism is to redefine the consensual goal of "equality" as the broadly supported "equal opportunity" for individuals, rather than the broadly opposed "equal outcomes for groups."

Ideologies, stereotypes, and explanations for inequality are all described as mere "cognitive embroidery" to pretty up dominant groups' pursuits of their real interests. In this view, they have no particular causal force in dominant groups' thinking, though they do serve the important propagandistic purpose of justifying the status quo to potentially rebellious subordinate groups. Dominant groups favor paternalistic policies, sugar-coating their resistance to change with positive feelings toward the subordinate group. Subordinate groups are somewhat more attracted to conflictual policies but have generally positive affect toward their presumed rivals and rarely dare to advocate radical change. So rival groups' policy goals usually differ only modestly. Even group stereotypes do not emerge with much vigor. All groups ascribed traits to their rivals only cautiously, accompanied by few invidious value implications, and usually consensually. For example, men and women were in general agreement on the nature of gender differences.

In all these respects—contact, feelings, perceived interests, policy goals, ideology, and "cognitive embroidery"—the three case studies yield about the same rank order: the most conflict is associated with race, the least with gender, and intermediate levels with class (mostly between very high and very low socioeconomic status). Some cracks do appear in the gentle mantle of paternalism and its accompanying "amicable consensus," principally due to the spatial segregation of the races, which allows blacks to disentangle themselves from the dominant whites both cognitively and emotionally, blunting the force of whites' individualistic ideology. In contrast, the close day-to-day contact of men and women more fully envelops their relationship in paternalistic initiative and deferential response.

Generally, Jackman makes a good empirical case, and the text typically reflects the data quite faithfully. Some may worry about the exclusive reliance on a survey conducted 20 years ago, perhaps missing such recent developments as the putative "year of the woman" in 1992 or "angry white men" of 1994. But I rather suspect that the findings will ring true to those familiar with contemporary survey data. Ordinary adults, viewed through the prism of survey research, usually do not express as much hostility toward other ordinary adults in presumably rival groups—or see as much intergroup conflict with them—as many social theorists think they should. However, she perhaps does overlook the readily tapped hostility expressed toward such symbolic advocates of change as "black militants," "feminists," "wobblies," or "communists," which might lead one down another path.

A larger question might be raised about the fundamental assumption that politics is animated primarily by the self-interest of large social groupings: "[The] assumption that attitudes are primarily the property of individuals is questionable. . . . [Rather, they] are the communal property of the group to which they belong" (p. 268). Yet the data show repeated denial of the "group basis of social life" by individual group members, with little evidence that they perceive widespread intergroup conflicts of interest, conflicting policy goals, or antagonism toward members of rival groups. Perhaps most ordinary people do not privilege their own class, race, and gender status above their many other group memberships and value commitments. This fundamental assumption is highly consequential, focusing the data analysis on between-group variance (which usually turns out to be rather small) and disregarding within-group variance (usually more substantial, blacks' consensus in some policy areas notwithstanding). The assumption risks exaggerating the "group basis of social life," especially these particular groups.

This is a book of major importance, challenging much conventional wisdom about race, gender, and class in American public life. The theoretical analysis is novel in many ways, even when dealing with relatively familiar findings. The scholarship is superb, and the exposition is both classy and lucid. It should become a central reference point in this literature.

University of California, Los Angeles DAVID O. SEARS

The Transformation of European Social Democracy. By Herbert Kitschelt. New York: Cambridge University Press. 1994. 345p. $54.95 cloth, $16.95 paper.

No simple patterns characterize the recent electoral fortunes of European socialist parties. There have been some rather obvious losers–the British Labour party (although this may be changing) and German socialists; and there have been some clear winners—the French socialists (this, too, is changing) and their colleagues in Spain. The overall pattern contains various ups and downs between adjacent elections, and the trend lines display indeterminate results. Placing electoral themes at the heart of the analysis of European social democracy and addressing several major theoretical issues (the utility of rational choice theory to the analysis of particular cases, the role of socialism in the contemporary world, and the emergence of new cleavages), Herbert Kitschelt offers a massively detailed and well-argued study that is an important contribution to the comparative analysis of political parties and party systems.

Kitschelt tests—and finds wanting—several well-known hypotheses of the determinants of socialist electoral fortunes: the simple and direct view of the relationship between class and vote; Przeworksi and Sprague's sophisticated analysis of trade-offs among social class, party, and vote; the relationship between a party's place in the government and opposition and its electoral fortunes; the consequences of economic performance, especially the presence of economic and retrospective voting and business cycles; and the effect of various structural arrangements of the political economy, alone and in combination. The analysis denies the explanatory power of each and all of these hypotheses.

In their place, Kitschelt offers a complex argument drawn from an extended version of rational choice theory and more general themes. Moving beyond "thin" definitions of rationality that view political parties as only maximizers of votes, he examines conditions under which parties might prefer to serve as "pivots" in a party system or to engage in oligopolistic competition. "Thick rationality" also includes the role of the internal organization and the party's ideological inheritance as factors that condition the strategic choice of party leaders. Kitschelt joins a powerful analytic tradition that underscores the active role of political parties in the creation of their electorates. In addition to the longstanding typology of political preferences that emphasizes trade-offs between liberty and equality, he posits a second dimension, fraternity, and ties this theme to the recent emergence of new left-libertarian issues. Kitschelt applies a complex theoretical apparatus to the question of variations in the electoral success of socialist parties.

Having provided demanding tests of alternative conceptualizations, measures, and explanations, however, Kitschelt is rather kind to his own ideas. The categorization of winners, "stabilizers," and losers rests on an inadequately defended selection of time periods and elections. Furthermore, by Kitschelt's own count, the Italian socialists (a winner, average gain between 3.2%) are more like the stabilizers (Belgian socialists +1.6%, the Dutch party +2.4%, and the Swedish party +1.3%) than the other winners (French socialists +9.5% and Spanish socialists +14.3%). The case histories of the nine socialist parties examined too often turn on the author's not-well-substantiated interpretations of critical political events. As a result, there is reason to doubt the summary assessments of the validity of the theory's predictions. In addition, the presentation of the left-libertarian cleavage rests on a factor analysis in which education plays too powerful a role, given the necessarily high correlation between education and white-collar/student positions and the long-standing willingness of the highly educated to claim to be willing to join social movements, whatever the groups' political colors. Furthermore, the analysis overstates the impact of issues on vote choice and provides a rather constrained view of ideas and values as things that deviate from decisions based on interest. In sum, a series of not-always-tenable assumptions and analytic decisions moves the study through critical empirical crossings.

Given the long history of the European socialist movement and the volume's general concern with its transformation, Kitschelt does not pay adequate attention to the decades before the 1970s and thereby misses the extent to which fluid electoral and preference patterns have always been present. He offers no fine-grained analysis of earlier periods. As a result, there is insufficient reason to accept one of the volume's major arguments about the future: powerful organizations necessarily limit a party's strategic flexibility. After all, the initial successes of the well-organized socialist parties occurred during decades in which most voters did not have enduring political or social class loyalties. Furthermore, it is not evident that crossing the equality-liberty dimension with a set of political preferences based on fraternity adequately captures the diversity of mass politics. Indeed, there are strong grounds on which to claim that most voters change their electoral choices and their views on political issues at least once during three adjacent elections. The high levels of electoral and issue fluidity cannot be contained in the static, two-dimensional model. Kitschelt's analysis does not demonstrate that strategic politicians, in any one country or across the diverse cases of European socialist parties, can devise a set of appeals that will substantially affect the electoral fortunes of their parties.

How successful is the analysis? At best, there is a plausible account in which the factors drawn out of an extended version of rational choice theory seem to be associated with variations in the electoral success of socialist parties during the 1980s. I would suppose that rational choice theorists will be very pleased with the effort; other political scientists will doubt that the results of the analysis justify or require the elaborate theoretical structure.

Brown University ALAN S. ZUCKERMAN

How Political Parties Work: Perspectives from Within. Edited by Kay Lawson. Westport, CT: Praeger, 1994. 317p. $59.95.

Once again, Kay Lawson has edited a book on comparative political parties with a punchy title and pithy contents. In her earlier and often-cited *When Parties Fail* (1988, with Peter Merkl), Lawson assembled a series of case studies describing organizations that replaced or supplemented traditional party organizations that failed. In *How Political Parties Work*, she offers a set of studies intended to "explore the internal dynamics of political parties with the intention of finding out how parties really work" (p. x).

Excluding Lawson's brief introduction and her substantial conclusion, the book contains 12 chapters neatly organized into four equal parts. The three chapters of part 1 deal with internal practices that impede or promote party democracy. Thomas Poguntke clearly explains how the German Green party managed to obtain a high degree of internal democracy but at the cost of efficiency in decision making and even at the impairment (ironically) of grassroots democracy. Geoffrey Debnam describes, less clearly, how the New Zealand Labour party created a layer of "policy committees" to bring the parliamentary party back into line after it had strayed too far in pushing its own political agenda. Analyzing six cases of French parties' efforts to regulate internal difficulties outside formal channels, Andrew Appleton divines five "informal rules" that guided their efforts.

Part 2 deals with the interaction between leadership and organization. Mildred Schwartz reports on her innovative "network analysis" of the Illinois Republican party, which uncovered such unofficial actors as "financial contributor" and "interest group advisor" within the party leadership. Drawing on his earlier insightful work, Kaare Strøm models party behavior as a simultaneous effort to seek three distinct goals—votes, offices, and policies—and examines data on six Norwegian parties to see whether different combinations of goal seeking explains differences in party organization. (Unfortunately, as Strøm recognizes himself, the Norwegian parties did not differ sufficiently on either goal seeking or organization to test the model adequately.) David Morris probes into the nature of factionalism in the Japanese Liberal Democratic party, with particular reference to the Komoto faction, contending that factions were not particularly well defined nor uniformly devoted to the factional leader.

Part 3, which examines the impact of internal organization on external performance, is probably the least cohesive section of the book. Subrata Kumar Mitra describes the transformation of the Indian National Congress from a programatic party to a catch-all party to one of personal authoritarianism. Working within the framework of contingency theory in the organizational behavior literature, Kris Deschouwer researches the impact of party organization on electoral and political effectiveness both across nations and among local party sections in Belgium. Drawing on the Israeli experience in the 1988 election, Yael Yishai introduces the innovative concept of an "interest party"—an interest group that chooses to try running candidates for legislative elections. If the group does so at the next election, she classifies it as a party. Of the five groups studied, only two became parties, and Yishai explains why.

Part 4, which I found to be the most rewarding, describes how parties change in response to external events. Two articles discuss how Gorbachev's reforms in the USSR impinged on communism in Central Europe. Jerzy Wiatr, a well-known Polish scholar even under the old regime, describes how the division between hardliners and moderates within the Polish United Workers party (PUWP) evolved into a split between hardliners and radical reformers that culminated in the historic party conference of January 1990. The conference began with a call for terminating the party. Then it recessed long enough to create a new party—Social Democracy of the Polish Republic—to which the PUWP's assets could be transferred before voting itself out of existence. Dealing with Czechoslovakia, Josef Blahoz focuses not on the dissolution of the Communist party but on the formation and dissolution of its democratic successors—Civic Forum in the Czech Republic and People against Violence in Slovakia. Blahoz contends that these broad antidemocratic movements should not have been expected to survive and that even subsequent party splits helped moved the two republics (now two countries) toward more democracy. In the last essay on changes in the social democratic parties of Britain, France, Germany, and Spain, Frank Wilson elaborates on his well-known model of party change, now stressing the importance of leaders' decisions about how parties should adapt to competitive challenges. Socialist leaders in France and Spain read the environment properly and modified their parties policies and tactics appropriately, whereas those in Britain (and, to a lesser extent, Germany) did not—with negative political consequences.

In her concluding chapter, "Toward a Theory of How Political Parties Work?," Kay Lawson considers alternatives posed by the *how* question: "Do we want to know how well the party works to achieve what the party itself is working for? Or do we want to know how well the party works for the system in which it operates? Or both? And what do we mean when we say, 'for the system'?" (p. 285). Although she recognizes all these questions as legitimate, Lawson chooses to define the problem for theoretical explanation as "how to make policy making parties, as opposed to power-seeking parties, work" (p. 287). Lawson creatively constructs a set of concepts that capture key ideas in the 12 articles, although none of the authors discuss their work in her terms. She argues, "How political parties work depends very largely on four variables: on the active members' motives; the means they employ; the boundaries they set, however temporarily, for the organization's identity; and the external situation in which they find themselves, as determined by the degree of power they have in government and the degree of stability of the system in which they function" (p. 299). On the last page, Lawson offers a set of prescriptions concerning how policymaking parties ought to work.

Unfortunately, Praeger allowed some flaws in the book's production. For instance, Deschouwer's chapter refers to regression coefficients that went unreported; Blahoz's notes are not properly matched to the text; and Lawson's conclusion contains a confusing typographical gaffe at the start of a paragraph. Although the collection of articles in this volume is probably too disparate for a text in a political parties course, scholars will value those parts relating to their interests and benefit from individual articles in this collection.

Northwestern University KENNETH JANDA

Building Democratic Institutions: Party Systems in Latin America. Edited by Scott Mainwaring and Timothy R. Scully. Stanford: Stanford University Press, 1995. 578p. $60.00.

The study of Latin American political parties can be dated largely from a handful of pioneering works appearing in the 1960s. With the subsequent wave of authoritarian regimes and the powerful if impermanent theorizing that accompanied the demise of democratic regimes, however, attention to parties was dramatically diminished. Now that at least formally democratic government has resurfaced, scholars are returning to research on parties. Perhaps the most ambitious undertaking is the collection under review here. Despite its merits, the work is ultimately disappointing, the whole being less than its constituent parts.

Although the initial effort to construct a unified and conceptually cohesive study dates from an October 1990 conference at the Kellogg Institute for International Studies, the result reflects the dangers and difficulties of multiauthored books. A comparison of the country-oriented chapters reveals a lack of organizational or analytic symmetry, and a number of the authors have ignored the thematic emphases set forth by the coeditors in the introduction. Some regurgitate far too much historical background. While others weave the history of parties into more contemporary events, there is an inclination to move from historical description to simply a more contemporary accounting of political events.

For a number of chapters, then, the contributions are relatively modest. For those seeking basic information about parties over time, there are such readily available source books as Alexander's *Political Parties of the Americas* (1982) and its companion work, Ameringer's *Political Parties of the Americas, 1980s to 1990s* (1992). More scholarly treatment for political and party-related events through the last decade are found in the appendices to Pastor's *Democracy in the Americas* (1989). This, in turn, increases authors' difficulties when, inevitably, their chapters are overrun by the latest of developments. The discussion of Venezuela, for one, penalizes itself by ending before the ouster of Carlos Andrés Pérez from the presidency, new elections, and the subsequent crisis of the parties. The conclusion of an otherwise perspicacious chapter on Colombia is affected by the electoral and other party-related events since 1991–92. To a lesser degree, the same criticism is valid for several other countries. Had the analysis been more self-consciously theoretical, these contributions would have been largely immune to such problems of up-to-the-moment accuracy.

Authors who proceed to develop their own theoretical (or, at the least, thematic) perspectives therefore provide the most valuable chapters in the volume. An outstanding example is the essay by Deborah J. Yashar on Costa Rica. After setting forth the atypical origins of that country's party system, she proceeds to link its evolution to that of an emergent welfare state. Social movements and the ideological leanings of Costa Rican classes are probed at length. The result is a more textured and conceptually refined analysis on the nation than one is accustomed to reading. Another stimulating discussion is the examination of parties in Ecuador by Catherin M. Conaghan. Recognizing that the critical period for the parties dates only from the 1970s, Conaghan properly concentrates on the most recent experience. This enables her to identify and detail major sociopolitical practices and attitudes that have prevented the party system in Ecuador from meaningful progress toward organized entities enjoying public support. The discussion is persuasive.

As a whole, the most useful chapter is Mainwaring and Scully's introduction, even if most contributors chose not to respond to their ideas. Several points need to be enumerated here. First, the emphasis is intentionally directed at party *systems*. For those seeking theoretical guidance for understanding the inner workings of political parties, this is not the place to look. Following Sartori's *Parties and Party Systems* (1976), the editors contend that an emphasis on systems marks a beginning point for comparative studies in Latin America. Moreover, the institutionalization of party systems provides the cornerstone on which theory building should rest. As they put it, "We above all wanted to provide a conceptual map for comparing Latin American party systems . . . and believe that examining levels of institutionalization provides a useful comparative framework" (pp. 33–34).

With party system institutionalization as their conceptual key, the editors elaborate five themes, to which they return in the concluding chapter. Among the most important are differences in the degree of institutionalization and the contention that party institutionalization is directly linked to the process of democratic consolidation. The first of these justifies their ordering of chapters. Part 1 is labeled "Institutionalized Party Systems" and includes six nations (Venezuela, Costa Rica, Chile, Uruguay, Colombia, and Argentina). Part 2 is "Hegemonic Systems in Transition," which contains chapters on Mexico and Paraguay. Part 3 contains the remaining four nations (Peru, Brazil, Bolivia, and Ecuador).

The second fundamental theme revolves about the relationship of party systems to democracy. The editors develop the notion that less polarized systems are conducive to democratic stability and the governability of the state. They set forth a definition that builds upon Sartori while incorporating a soupçon of Huntington. Suffice it to say that after first considering how systems affect political outcomes, the conclusion looks at parties "as dependent variables shaped by new developments in society" (p. 459). Economic variables are also factored in as critical to the new environment facing contemporary party systems.

Building Democratic Institutions, then, serves to provoke greater and more theoretically refined work on Latin American parties. Mainwaring and Scully themselves raise important issues, speculate thoughtfully about explanations, and generally proffer heuristically rich issues to be explored by present-day stasiologists. The individual chapters are academically sound but in too many cases reiterate material that is readily available elsewhere or already familiar to experts on the countries in question. Only a few present anything new or original. For students of both parties and party systems as focused on individual countries, a richer sampling is available in Perelli, Picado, and Zovatto's *Partidos y clase política en América Latina en los 90* (1995).

Pennsylvania State University JOHN D. MARTZ

Unruly Order: Violence, Power, and Cultural Identity in the High Provinces of Southern Peru. Edited by Deborah Poole. Boulder: Westview, 1994. 285p. $52.50.

The spectacular rise of the Sendero Luminoso (Shining Path) guerrillas in Peru sparked great interest among scholars not only in political science but also in other disciplines, including anthropology. Anthropologist Deborah Poole emphasizes in her edited volume that political violence has been a permanent feature of Andean life—but a feature that has received insufficient attention from anthropologists. In her final chapter for the volume, Poole looks to peasants' interpretations of political violence for clues to the intriguing question of why some peasants supported the Shining Path movement.

Poole assembles three other anthropologists, a geographer, and a historian to analyze the historical role of violence in the Peruvian highlands and the impact of the history of political violence upon the culture of highland peasants today. The regional emphasis is upon the "high provinces" (provinces at high altitudes) in the departments of Cusco and Apurímac, in particular upon the province of Chumbivilcas. In the first chapter of the volume, geographer Daniel W. Gade describes patterns of settlement and land use in these high provinces, emphasizing their isolation.

Two interesting chapters based on events in these provinces are provided by anthropologist Benjamin Orlove. In one chapter, Orlove describes a series of killings in a Cusco community in 1931: after a group of *mestizos* slay a peasant who attempted to escape arrest, two of the *mestizos* are seized and killed by Indian peasants; one week later, police travel to the community, not only arresting dozens of peasants but also setting fire to the village. Orlove analyzes the subsequent distortion of these events in the newspapers of the city of Cusco—in particular, the portrayal of the peasants' spontaneous act of revenge as a purposive, savage, communist "uprising." In his second contribution to the volume, Orlove describes "ritual battles"—battles that are restricted to certain dates in the religious calendar and in which the outcome of the battle is not a primary concern of the inebriated participants but in which serious injuries are sustained. Orlove suggests that the participants' key purpose is to demonstrate both to themselves and to outsiders their willingness to fight.

Another valuable contribution is made by historian José Luis Rénique, who traces agrarian politics in highlands Puno in the wake of the 1968 agrarian reform. Rénique shows how the expectations of the vast majority of Puno peasants for the reform were dashed and how leftist political movements as well as nongovernmental organizations seeking further land redistribution grew in the area during the 1980s. Elected in 1985, the Alan García government promised further land redistribution, but the national government's initiatives were blocked by local Puno elites. Ultimately, in the late 1980s, these longstanding peasant frustrations were exploited by the Shining Path.

Ironically, however (given editor Poole's interest in the question of peasant support for the Shining Path), Rénique's chapter is the only one in the book that focuses upon an area of Peru where these guerrillas became strong. Chumbivilcas, which is the focus of the chapters by Poole and Christiane Paponnet-Cantat, is located in Cusco. As Gade points out, "The Cusco region has been relatively quiescent" (p. 53). Accordingly, the relationship between the political violence described in most chapters of the volume and the emergence of the Shining Path is vague; the extent to which patterns in extremely isolated Chumbivilcas might or might not be representative of most of the Peruvian highlands is never indicated.

Chumbivilcas appears to me to be a very unusual community. A key argument in the chapters by Poole, Paponnet-Cantat, and Peter Gose is that the *gamonales* (traditional landed class) may have been ousted from their highlands estates by the agrarian reform but that this class continues to wield great power over the peasants. States Paponnet-Cantat: "[There is] a *gamonal* class whose post-reform power has been enhanced. . . . [The *gamonales*] continue to subject the *comuneros* to the same onerous and pervasive exploitation inherent in the pre-reform system" (p. 218). In particular, in Chumbivilcas, former *gamonales* apparently sought to reassert their power by encouraging their children to become rural schoolteachers. Given the hardships of life in Peru's high provinces and the low salaries for teachers, the predominance of former *gamonal* families among the ranks of schoolteachers was anomalous; the much more common pattern was that schoolteachers were to the political left. Indeed, as Rénique indicates for Puno (p. 240), schoolteachers formed a core group of support for the Shining Path in many areas. My concern about the argument by Poole, Paponnet-Cantat, and Gose is that these anthropologists are making the error cogently criticized by Orin Starn in his 1981 article, "Missing the Revolution: Anthropologists and the War in Peru": their conceptualization of *comuneros*, *mestizos*, and *gamonales* as discrete social categories is outdated.

In the final chapter, Poole provides her own interpretation of the rise of the Shining Path in Peru's southern highlands. Although her argument is not entirely clear (concepts are not defined, and different analytical threads are not neatly woven together), Poole seems to contend that it is the *gamonales* and other provincial elites who are responsible for the political violence; that it is from their ranks that the Shining Path leaders originate; and that overall, the violent Shining Path doctrines were rejected by *comuneros*. Peasant support for the Shining Path existed but was fleeting; support was conditioned upon the efficacy of political violence in resolving specific local grievances.

In various respects, I agree with Poole's interpretation. But the analysis relies on the outdated dichotomy between *gamonales* and *comuneros*. It also underestimates the length of time for which the Shining Path generated support in some provinces, as Ronald Berg described in "Sendero Luminoso and the Peasantry of Andahuaylas" (1986–87). Further, the argument exaggerates the perspicacity and rationality of peasants, who in this interpretation are correctly discerning exactly when violent actions will or will not be effective (but at the same time are oblivious to the very real threat of military reprisal). In Poole's discussion, the actual voices of Peruvian peasants—which I frequently heard expressing despair, rage, and fear—are strangely muted.

Nor does Poole's interpretation jibe well with other chapters in the volume. Both Orlove's chapter describing ritual battles and, ironically, Poole's own chapter portraying a particularly bloody type of bullfight that is a ritual in Chumbivilcas imply a degree of acceptance of violence among highlands peasants. In her chapter, Poole concludes that the bullfight is an example of the "hegemonic project mounted by intellectuals with ties to the *gamonal* class" and that it shows the success of this "otherwise reviled and abusive elite" in forging "the identity of an entire province" (p. 125). Such an interpretation appears an exaggeration in many ways.

Greater interest in political violence among anthropologists can only be welcomed among political scientists, and Poole's volume is a positive step in this respect. However, if the editor had been more careful in her selection of cases so that they be pertinent to her concluding argument and more nuanced and rigorous in her conceptualization, the book would have been a more significant contribution.

George Washington University CYNTHIA MCCLINTOCK

Media and Revolution: Comparative Perspectives. Edited by Jeremy D. Popkin. Lexington: University Press of Kentucky, 1995. 246p. $29.95.

The 13 essays in this book, written mostly by historians with a sprinkling of journalism and literature professors, explore the role played by mass media in the unfolding of revolutionary situations. The inquiry spans nearly 350 years—evidence that media coverage of human upheavals has a lengthy track record. The scope of the essays varies, ranging from a close-up examination of the impact of a single journal's coverage on an ongoing revolution to a panoramic view of the interaction of entire media systems with evolving revolutionary events.

What conclusions do the authors reach about the role played by the media in revolutions? Not surprisingly, there is no agreement. The assessments they make about media influence run the gamut from considering the media central to revolutionary processes to deeming them peripheral. For example, Pierre Rétat, professor of literature at the University of Lyon II in France, and Jeremy Popkin, professor of history at the University of Kentucky, contend that the press played a crucial role during the French Revolution, even if it was not its cause. By contrast, historian Jeffrey Wasserstrom and journalism professor Owen Johnson, both of Indiana University, downplay the media's role in 1989 during the Tiananmen Square demonstrations in China and during Czechoslovakia's "Velvet Revolution." The appraisals of the other authors fall into the middle ground between pleading a strong influence and belittling media impact. They cover revolutions and their aftermath in England, America, and Russia during various phases of each nation's history.

It would be surprising if analysts of diverse major political upheavals agreed about the role played by news media. The case studies assembled in *Media and Revolution* represent a motley array of news sources covering successful and failed revolutions in dissimilar countries during different historical periods. Media impact is bound to vary in particular revolutionary contexts and when media diverge in content, style, and prestige. A second reason is the extreme difficulty of isolating the impact of media when they are only one of many factors at play in the complex political situations involved in revolutionary upheavals. Thus far, most attempts to measure the precise impact of media on political outcomes have failed because the dynamic interplay of forces in the real world cannot be measured with precision. Simulations and experiments help very little precisely because they lack the complexity that confounds real-life situations. Finally, it would indeed be highly unusual if 13 academics drawn from different intellectual and ideological backgrounds, with disparate preferences in research design and methods, could reach a consensual judgment.

Nonetheless, there are some areas of agreement. The essayists concur that revolutionary crises produce changes in media systems, such as a mushrooming of media enterprises, the entry of new voices into the media chorus and the exit of some of the previously dominant groups, and substantive changes in media content. Most of the essayists contend that these changes are generated by the same social tensions that sparked the revolution. Contrary to prevailing notions about the media's influential prerevolutionary role, they shun claims that the media caused the revolution or, at the very least, served as kindling for the revolutionary pyre. Rather, they argue that the media play their biggest role while the revolution is in progress and during postrevolutionary reconstruction.

The essays that make up the book were first presented at a conference held at the University of Kentucky in 1992. As is common in such symposia, they are uneven in quality; but readers are unlikely to agree on rankings. Among my favorites is an essay by Jeremy Popkin, the volume's editor, that traces varying perceptions of the anatomy of revolutions over time. It points to the gap between the views of revolutionaries and the opinions of scholars. For example, Popkin quotes Lenin's views about the importance of newspapers in fomenting revolution and Mirabeau's belief that the fate of the French Revolution hinged on the existence of a free press. By contrast, until very recently, most historians made light of the role of the press, blaming revolutions primarily on factors such as social structure, political leaders, ideologies, parties, and movements. He credits scholars such as Jürgen Habermas in Germany and Roger Chartier in France with the paradigm change that led scholars to focus on the degree of centrality of the media in revolutions.

Another essay that deals with political science concerns is Tim Harris's study of propaganda and public opinion. Harris, who currently teaches history at Brown University, compares the efforts of the seventeenth-century English Parliament to mobilize public opinion against the Crown with the successful counterpropaganda launched by the Tories in the early 1680s. His research sheds light on the interactions between the values prevalent in a society and the efforts of opposing propagandists to lay claim to these values while trying to change them in antithetical ways.

Two other favorites of mine are Thomas Leonard's "Antislavery, Civil Rights, and Incendiary Material" and Mark W. Summers's "American Cartoonists and a World of Revolutions, 1789–1936." Leonard, dean of the Graduate School of Journalism at the University of California at Berkeley, compares the attacks on northern abolitionist newspapers in the 1830s and on reporters and photographers covering civil rights issues in the 1950s. He concludes that the violence against institutions that were perceived as antiracist had important social consequences, which often counterbalanced the thrust of their stories. Summers, a University of Kentucky historian, demonstrates with well-chosen cartoons that revolutions were depicted as evil in the United States during the nineteenth and early twentieth century. This stance matched the views of the media's owners, as well as the antirevolutionary sentiments of most average Americans.

Overall, these are well-written, interesting essays on an important and timely topic. But to use a culinary metaphor, in their present form, they serve as hors d'oeuvres for the mind that whet the appetite for a fuller, more balanced meal, rather than the full meal itself. The chefs seem eminently qualified to go back to the kitchen and bring forth a sorely needed feast.

University of Illinois, Chicago DORIS A. GRABER

Organizing Societies for War: The Process and Consequences of Societal Militarization. By Patrick M. Regan. Westport: Praeger, 1994. 208p. $55.00.

Regan argues in *Organizing Societies for War* that societal militarization is a complex political and sociological phenomenon and analyzes the potentially self-amplifying feedback relationship between societal militarization and violent foreign policy. He combines statistical and historical-empirical analysis, in seven chapters, to develop a theory of political symbolism's power. In exploring Lasswell's thesis that nationalist symbols are manipulated in order to gain public acquiescence, Regan asks whether violent foreign policy feeds the process that leads to a militarized society. Is a highly militarized society more likely to resort to violent diplomacy? Is there a feedback relationship between violent state behavior and societal militarization? Does a political culture or myth reify the military and bolster threat perceptions in order to support mobilization? What are the socialization mechanisms through which citizens accept and participate in mobilization?

Regan's empirical analysis concentrates on the United States and United Kingdom (as similar countries and systemic leaders) from 1900 to 1985 but also includes a historical case study of Brazil, a country with a large military-industrial complex, to broaden the generalizability of his findings. A militarized society has a large part of the public organized—and socialized—by elites for war preparation such that civilian-military differences are blurred. He measures the number of active and reserve forces, the number of civilians working for the military, the number of veterans in veteran organizations, the number of civilians working in the military hardware production, and the number of secondary and university military training programs. In the *militarization* process, states move from a less to a more militarized condition, while *militarism* refers to military-based values. The former is catalyzed, in part, by technological development, which is measured by a composite of iron-steel-aluminum production and computer equipment sales.

Regan also reviews C. Wright Mill's distinction between elite and public, differences between a militarized and nonmilitarized society, the links connecting threat perceptions with domestic economic-political constituencies, and the insecurity dilemma. He outlines the existing militarization literature concerning traditional societies, newly industrialized countries (NICs), and industrialized societies before discussing the five hypotheses he will test. The hypotheses are as follows:

1. Increased militarization leads to more violent foreign policy by the state.
2. Increased violent foreign policy leads to societal militarization.
3. Highly militarized societies tend to be industrialized.
4. A more militarized society tends to maintain a threat perception in the media.
5. Cultural symbols that help create/maintain war preparations are more evident during high mobilization.

The first two outline the self-amplifying feedback relationship between societal organization and international behavior, while the fourth highlights how scholars should consider why the Cold War's end has not led to decreased Western concern about future antagonists. It would be interesting if future research supports Regan's findings and approach when applied to NIC military-bureaucratic-authoritarian regimes.

Chapter 2 examines how political-economic inertia, technological development level, existential threat, perception of external threat, and violent foreign policy all contribute to societal militarization. He measures the extent to which the civilian sector is organized around the production/preparation for war by calculating the labor-force percentage involved and tests his first three hypotheses. It would have helped the lay reader had Regan explained why a multiple regression model with least squares estimator was considered appropriate. His military-civilian participation ratio and militarization graphs suggest that militarization is strongest during periods of war, arms races, and Cold War. In the post–Cold War era, it would be interesting to test whether threat perception manipulation to maintain public support is still prevalent. Regan's initial findings weakly support his argument that militarization is influenced by interstate disputes, technological development, threat manipulation, and existential threat, although evidence is stronger for the United States than the United Kingdom.

Next, he discusses how societal militarization, clarity of international order, and system-bloc leadership help explain violent foreign policy. Beyond the putative link between societal militarization and violent foreign policy, Regan hypothesizes that greater systemic concentration would lower the propensity to use force and that system leadership is associated with conflict propensity. Here Regan could explain to the lay reader why a linear regression model with least squares estimator was appropriate. Initial U.S. and UK results do not support militarization and concentration as factors exacerbating dispute involvement, although there is some support for leadership. After his model is revised to include changing militarization levels, his U.S. evidence (but not the UK evidence) is slightly stronger. Deleting World War II (to account for wartime mobilization) from his analysis strengthens his U.S. results but not his UK findings, and it is unclear why World War I was not deleted. The differential impact of postwar demobilization could help explain why the United Kingdom demobilized more extensively than the United States. Regan could also have discussed the militarization–global tension dynamic's implications for the insecurity dilemma.

Regan also uses a simultaneous equation model, with two-stage least squares estimator, to test the feedback relationship between militarization and violent foreign policy. This feedback is driven by the need to rationalize existing preparedness, international tensions on attitudes or perceptions, and the military's societal role. His dependent variables are the number of ongoing disputes at a given time and the change in militarization, while his independent variables include lagged militarization, logged industrialization, threat perception manipulation, existential threat, system leadership, and concentration. His U.S. findings suggest that the feedback between militarization, use of violence, and ongoing disputes is not self-amplifying but contradictory. But his UK results suggest the presence of a weak feedback and that systemic leadership and technological development are negatively related to both disputes and militarization. Greater concentration leads to fewer disputes and lower militarization, for both states; but there seems to be a negative relationship between previous and current militarization levels. If similar results occur when Regan's model is applied to other industrialized countries, then his findings would have greater weight.

The chapter on how societal symbols, measured by war toy and movie production, help inculcate societal militarization is not well integrated with earlier analysis. Regan attempts to show that press reports of threats and war symbol production covaries with militarization. Public attitudes toward the military, war, or threat perception are reflected in war symbol consumption/production. But Regan should also have considered television and radio's socialization roles, defined *security elite* more clearly, and considered whether a media popularizing militaristic symbols can be considered liberal. His findings suggest some correlation between militarization and press stories, toys, and movies in the United States but not for Britain. The Brazil case study is qualitative in analysis and begs the question why such an approach was not applied to the United States and United Kingdom to supplement quantitative analysis. Changes in Brazil's society and military suggest the need for similar analysis in other case studies. At least for

NICs with current or past military-bureaucratic-authoritarian regimes, the link between industrial development and militarization, as well as the possible presence of a militarization threshold, is worth exploring. The final chapter summarizes Regan's findings but some of the generalizations regarding societal attributes could be more qualified. His suggestions for demilitarization raises the question of who will push for attitudinal changes in a militarized society and how they will do so. Overall, political sociology and international relations graduate students would find chapters 1–4 and 7 most useful.

Queen's University ALLEN CHONG

Inheritance in Public Policy: Change without Choice in Britain. By Richard Rose and Phillip L. Davies. New Haven: Yale University Press, 1994. 256p. $34.95.

Rose and Davies have written a book that rests on substantial and careful research. Equally important, they have written a book based on an intriguing idea—yet another in the long series of intriguing ideas with which Richard Rose has been associated.

The idea on which Rose and Davies's book is based is that the vast majority of what governments do is based on programs they have not chosen, because they have inherited them. The inheritance of a government is more important than the choices it makes. The priorities of government today reflect not choices made today but choices made by a government's predecessors. Some of these predecessors are distant. Rose and Davies show that when Mrs. Thatcher moved into Downing Street in 1979, over half the laws in force predated World War II, and over one-third predated the start of the century. What Rose and Davies term "continuing inheritance" accounted for 87% of public expenditure under Mrs. Thatcher. Many old, inherited commitments, like Social Security expenditures in the United States, have grown disconcertingly rapidly, so that contemporary governments are faced with financial difficulties for a program they themselves did not choose to create.

Rose and Davies base their argument on evidence drawn largely from Britain. This choice was determined in part by the impressive record keeping of British government (a fine tradition endangered by recent Conservative governments). Yet, though Rose and Davies praise the quality of the records they used, the authors were required to engage in careful and tedious reconstruction of official records. One of their simplest questions about the statistics they used required a three-page response from the Treasury. Like the proverbial iceberg, two-thirds of the work for the book is below the water line, invisible but crucial.

Rose and Davies claim in their subtitle that this is a book about Britain. So it is in terms of the evidence used. Those interested in British government will learn from the book; the heroic leadership style of Thatcher and fears of "elective dictatorship" or "adversary politics" in Britain may have given observers the impression that everything in British government is unstable. This book is a useful corrective to such fears. Even under Thatcher, there was more stability than change in government. Readers interested in British government will also be delighted by the argument that a significant proportion of programs abolished by British governments were programs that government had introduced itself only a few years earlier. Clearly, the poll tax (community charge) was not an isolated example of a program regretted quickly even by its parents. Yet as Rose and Davies make clear, their argument is intended to be a contribution to the study of public policy more generally. Indeed, a short comparative section argues that the authors' argument applies equally well to the United States with its very different system of government.

How convincing is the central idea of inheritance before choice and of change without choice? As linguistic philosophers whom Rose might have met in his Oxford days would be quick to say, it all depends what one means by "choice." Clearly, Rose and Davies are correct in pointing out that most of the programs operated by government were not *chosen* by the government now in power. Yet, very often, governments are happy with the choices they have inherited.

There are various forms of policy inheritance. First, there are inheritances that are approved of entirely. We can assume that contemporary governments in Britain (and elsewhere) are pleased that a previous government went to the trouble of making murder or theft crimes. Second there are inheritances that are welcomed, though only in private. It might well be that Mr. Blair's Labour government (expected confidently and perhaps longingly by so many) will be glad (though it will not say so) to inherit labor laws from the Conservatives that have, at long last, brought rationality to British labor relations. Third, there are inheritances lost in the governmental attic of which contemporary ministers may be unaware. No one notices the expense or effort involved. The practice of stationing a guardsman on the steps leading down from Pall Mall to the Mall was perhaps a case in point, a practice caused by an incident lost in memory (a child's pram rolling down the steps) with consequences that endured for decades. There are probably fewer such examples in the budget-strapped 1990s than in the past. Finally, there are disagreeable inheritances—unfashionable furniture—that cannot be discarded, however much one might wish to. Such unwelcome inheritances in government are, ironically, often very expensive programs with too much political support behind them to change. Thatcher was forced by politics to maintain (and even expand) the national Health Service. But what is and what is not politically possible changes, partly in response to the values of the government in power. It was once thought impossible politically to change British labor laws and inconceivable to privatize nearly all government-owned industries.

Governments have limited legislative time and political resources with which to make changes. But even so, might it not be argued that in most of the forms of inheritance we have distinguished, governments may be described as *accepting* programs they have inherited from the past, if not actually welcoming them? Programs do not continue to exist in isolation from politics and society. Rather, programs endure supported by political, ideological, and social forces. Some of these coalitions may be large (the National Health Service in Britain or Social Security in the United States), others small (e.g., the famed mohair subsidy). They are large enough, however, to enable a program to persist until overwhelmed by superior political, ideological, or social forces. It is always wrong to say that *no* means *yes* and often wrong to say that silence equals consent. Yet old programs persist on the sufferance, if not enthusiastic choice, of contemporary governments. Inheritance can be a form of choice. After all, one can always give that furniture to Goodwill.

University of Wisconsin, Madison GRAHAM K. WILSON

Manufacturing Militance: Workers' Movements in Brazil and South Africa, 1970–1985. By Gay W. Seidman. Berkeley: University of California Press, 1994. 361p. $45.00 cloth, $15.00 paper.

For those interested in the political economy of development and underdevelopment, Brazil and South Africa have long provided fascinating cases for examining the complex and multidimensional features of "late industrialization." For those concerned with the rise of widespread demands for democracy across the world in the late twentieth century, Brazil and South Africa again furnish important cases of political transformation. In a carefully constructed analysis comparing these countries from the late 1960s through 1985, Gay Seidman argues convincingly that structural changes resulting from rapid industrialization gave rise to a new form of labor organization, *social-movement unionism,* that played a key role in shaking the foundations of authoritarian rule.

Demonstrating "multiple and conjunctural causation in case-oriented research," as Charles Ragin advocates in *The Comparative Method* (1987), Seidman pays close attention to the specific contexts and peculiarities of each country. Yet she examines these details and complexities within a framework that provides a bigger picture of underlying structures and processes, a framework that explains why union mobilization occurred when and where it did and why it had such significant consequences.

Brazil and South Africa both experienced rapid industrialization and high growth rates in the 1960s, primarily a result of *associated-dependent* development strategies—a term coined by dependency theorists on whose conceptual foundations Seidman's analysis partially rests. Although the political economies of these countries differ in important respects, in general, both states built on previous import-substitution policies of the 1950s to further assist industrialization through (1) increased investment in basic industries; (2) strong incentives for multinational and domestic investments in new industries, "deepening" the industrial base; and (3) severe restrictions on labor mobilization, keeping wages well below increases in productivity.

Elite cohesion in support of these policies increased as the economy continued to grow. The skill level of industrial workers also grew significantly, making them increasingly valuable to their employers.

The 1973 oil crisis, however, revealed the "shaky foundations on which these 'economic miracles' were built" (p. 90), bringing on protracted economic difficulties and hastening an end to elite consensus about high levels of political repression and particular economic policies.

The 1970s also brought major challenges to industry and the state by militant labor unions with widespread support in their communities. These groups took advantage of the political space created by cracks in elite cohesion to press their demands for better wages and living conditions. Yet how—after decades of severe repression that continued throughout this period—did opposition groups find opportunity not only to organize themselves but also to win short-term gains and later to become leading forces for subsequent dramatic political transformation?

Seidman demonstrates the structural change that provides part of the explanation. At the same time that industrialization transformed the economies of Brazil and South Africa, it also reconfigured the working class, changing both its composition and its capacity for organization. The postwar spread of multinational capital organized production for a global market and employed new processes that used semiskilled workers and imported capital-intensive technologies. Despite high unemployment, industrialists in key sectors became reluctant readily to sacrifice their fairly young, relatively well educated, semiskilled and skilled workers to the states' efforts to crush labor organization.

When large strike waves suddenly interrupted a decade of industrial quiescence in both countries (in 1978 in Brazil and 1973 in South Africa), employers groped toward responses that would retain, rather than dismiss, workers.

Under pressure from abroad and in hopes of coopting workers' organizations, industrial elites pressed the state for controlled reform and liberalization. Seidman concludes that "although they were formed in opposition to 'savage capitalism', both workers' movements were shaped by the processes of that capitalism, which changed the relationship between employers and the state" (p. 263).

Thus, the labor unions forged in the 1970s emerged from industrial circumstances different from those encountered by labor groups preceding them in their respective countries. They also grew strong within geographically concentrated communities whose living conditions had deteriorated substantially during the countries' economic boom periods. Rising from the status of underground clandestine networks to become large social movements, this "new unionism" moved beyond narrow shop-floor concerns to embrace wider political demands for their communities (i.e., demands for better housing and transportation, land reform, educational opportunities, and health care). As new participants in heavy industry and as longstanding community organizers, women played significant roles in linking the shop floor to the nearby neighborhoods. This social-movement unionism then continued to grow and become a key player in political transformations that moved both Brazil (in the 1980s) and South Africa (in the late 1980s and early 1990s) away from authoritarianism and toward democracy.

Seidman demonstrates understanding of a wide range of scholarly and theoretical literature related not only to political economy but also to social and political change, social movements, gender studies, and labor movements. She also knows her cases well, displaying considerable skill and depth in her research. At one point, she makes a refreshing plea for nondogmatic approaches to critical understandings of the dynamics and consequences of capitalism.

Seidman combines primary and secondary sources to construct her argument, using interviews with participants, documentation from their meetings and organizations, and government documents, as well as a range of other scholars' data and writings about the events that transpired. In this regard, her evidence at points is stronger for Brazil than for South Africa, where the threat of political suppression continued to inhibit thorough research at the time that the study was undertaken. Seidman notes this problem briefly in her introduction, and only occasionally does she lapse into drawing conclusions too thinly supported by data.

The study demonstrates the extreme difficulty of reconstructing interpretations of events that occurred during periods of extreme repression and high politicization in both countries. Yet Seidman makes a convincing argument through explicit and careful construction of a theoretical explanation that she systematically interweaves with credible information about these cases. Stylistically, the cost of such a close hermeneutic account with fairly frequent summations is a book that becomes long and redundant at points. But the writing is clear and smooth and thus very readable.

In short, Seidman displays comparative social science at its best, giving those with area specialties in Africa and Latin America (and Asia) good reason to follow her example in continuing the search for theories that travel across time and place without sacrificing depth, detail, nuance, or complexity.

University of South Carolina JANICE LOVE

Making Workers Soviet: Power, Class, and Identity. Edited by Lewis H. Siegelbaum and Ronald Grigor Suny. Ithaca: Cornell University Press, 1995. 399p. $49.95 cloth, $19.95 paper.

This collection of essays is a useful introduction to the field of Russian and Soviet labor history. The 14 chapters range over the period from the 1870s to the 1930s and are written by the leading specialists in the field.

Over the past 20 years, a rich corpus of work has been assembled documenting the rise of an industrial working class in the late tsarist era. Several dozen monographs have been written analyzing its rise to political maturity through the tests of 1905, 1917, the civil war, and the first five-year plan. Thanks to this body of historical writing, we now know far more about the lives of Russian workers in the first decades of this century than we do about Soviet workers in the 1950s or even 1980s.

Yet Soviet labor history has now entered a period of self-doubt and crisis. Most of the authors in this collection come out of a loose Marxist tradition, in that they started from the assumption that industrial workers formed a nascent class that was be a vital agent of historical change. They challenged the established tradition of historiography of the Bolshevik regime, which focused on the state as the main instrument of change (collapsing under the tsars and being reassembled by the Bolsheviks) and stressed Russia's uniqueness, rather than its exposure to common processes of historical development.

Scholars of Russian labor now face a challenge on two fronts. First, the collapse of the Soviet state has cast doubt over the validity of Marxist analysis in general and its grounding in Soviet history in particular. Second, historians of Russia had largely avoided the new forms of writing social history that have emanated out of Paris over the past two decades. The new history stressed discourse analysis over the documentation of social and working life and looked for multiple and shifting identities, rather than evidence of the unfolding of class allegiance.

In their opening chapter, "Class Backwards: In Search of the Soviet Working Class," the editors Lewis Siegelbaum and Ronald Suny acknowledge that the old approach has outlived its usefulness. They concede that "class promised social analysts a precision and certainty that it was never able to deliver" (p. 3). Rather than seeing class as the expression of a social movement swelling up from below, they acknowledge that "we might do better to conceive of class as an emerging political construct whose employment signified loyalty to the regime" (p. 19); that is, the rhetoric of class identification was forced upon the society by the state.

This sounds suspiciously like the "top-down" history of the traditional authors whom the labor historians had previously excoriated. However, old habits die hard; and despite the mea culpas in the introduction, the ensuing articles in this collection do not really break from the established paradigm. Most of the chapters are still concerned with differentiating between Russian workers in terms of traditional Marxist categories, such as level of political consciousness or degree of skill. Nevertheless, the reader can pick up much fascinating and informative detail from these thoroughly researched and nuanced essays.

The focus is very much on the work place, and nonclass forms of identity get short shrift. Chris Ward's chapter on the cotton industry concentrates on loom technology and devotes only one page to the issue of gender (p. 206). Hiroaki Kuromiya's chapter on the Donbas miners does not discuss whether they were Russian or Ukrainian (although it does make passing reference to conflicts with Jews and Tatars). Much of the analysis centers on the traditional Marxist concerns of organization and resistance, while the logic of the ruling class tends to be taken as given. Sandy Smith, for example, talks about "the foreman as the agent of capital" (p. 127).

Daniel Orlovsky investigates the role of white-collar workers, and notes "the tremendously rich variety of social and occupational groups that emerged in late Tsarist Russia" (p. 224). He argues that the state adopted the rhetoric of class in order to absorb these diverse and stratified social groups under its control (p. 248). Gabor Rittersporn and Stephen Kotkin stress the rapidity of the transition from peasants to workers, although their focus is still on the labor process (chaps. 9–10). Sheila Fitpatrick looks at the role of workers in the purges (chap. 11), and Victoria Bonnell analyses representations of workers in political art (chap. 12).

What does this work mean for political scientists? By deconstructing the Soviet working class as an agent of history, labor history may be in danger of becoming less relevant to political scientists trying to understand the trajectory of the Soviet state. These historians seem reluctant to come up with a new conceptual framework to assist nonhistorians in making sense of the past. Ironically, this coincides with a revival of interest among political scientists in historical topics, as reflected in the works of historical sociologists such as Theda Skocpol, Martin Shaw, Charles Tilly, and others.

Thus, while our understanding of the genesis of the American and European state is waxing, the contours of Russian historical development seem to be receding into a murky and elusive past. This cloud may have a silver lining, in that political scientists are obliged to jettison their simplistic summaries of Soviet history, while historians are finding rich new fields of study opening up to them.

Wesleyan University PETER RUTLAND

Race and Culture: A World View. By Thomas Sowell. New York: Basic Books, 1994. 352p. $25.00.

The reexamination of the nature of apparent differences among racial and ethnic groups—and the implications of these differences for public policy—has recently attracted widespread attention. The intense public discussion (including a level of mass media coverage seldom accorded issues of scholarly interest) that followed publication of Herrnstein and Murray's *Bell Curve* (1994) marked an important shift in the terms of the debate. Coinciding with judicial and political attacks on affirmative action and related policies designed to reduce race-based disparities, there has been a resurgence of conservative scholarship seeking to redefine prevailing ideas about race in American society.

Thomas Sowell figures prominently among these conservative voices. His explicit intention in *Race and Culture* is to "challenge many dogmas of so-called 'social science' " (p. ix). Sowell identifies as a particular target the idea that various differences observed across racial and ethnic groups are due to artificially imposed inequalities resulting from discrimination and other invidious forces of oppression. He also disputes the corollary assumption that the best or only solution to racial/ethnic inequality is to institute policies designed to protect or compensate racial or ethnic groups who would otherwise experience below-average outcomes. Sowell sees differential intergroup outcomes as a result of cultural factors that have an existence largely independent of—and more important than—immediate social or economic circumstances. *Race and Culture* is an extended attempt to build support for this general thesis using arguments and examples drawn from a broad range of cultures and nations (hence the book's subtitle). The discussion draws on a large number of studies conducted by sociologists, political scientists, anthropologists, and psychologists. Although the preface of the book refers to Sowell's visits to various sites around the world in the course of researching the topic, there is no original data presented in the text.

Race and Culture addresses the implications of ethnicity and culture in migration, conquest, economics, politics, intelligence, slavery, and history. For each topic, Sowell introduces some cross-cultural comparisons, although there is a great deal of variation from chapter to chapter in the quantity and depth of the cross-cultural data. The chapters on migration and conquest both address the question of cultures and peoples mixing, whether voluntarily or by force. In analyzing immigration, Sowell emphasizes the importance of economic benefits that many immigrant groups have brought to their host societies, particularly where the indigenous cultures lacked features important to economic development, such as entrepreneurial energy, experience in a particular industry, or the simple willingness to work hard. This latter theme is developed and further elaborated in the chapter "Race and Economics," in which Sowell contends that patterns of economic inequality across racial and ethnic groups are ultimately traceable to differential valuations determined by market forces. Sowell thus argues that where arbitrary discrimination exists, employers will ultimately pay the price in terms of lower productivity or reduced profits (p. 88). The only exception is where "a particular group is less productive than others, but individuals within that group are just as productive as anyone else. Here the cost of discrimination must compete with the cost of knowledge" (p. 89). Using the same market-based logic, Sowell contends that attempts by government to help (e.g., by minimum wage laws or antidiscrimination measures) only serve to make the situation of minority groups worse. Sowell's skepticism about any possible good coming from government intervention is underscored in the chapter "Race and Politics." Here Sowell attempts to demonstrate that the emergence of ethnic-based politics both threatens to exacerbate divisions in the society as a whole and is unlikely to produce long-term benefits for disadvantaged groups.

Sowell's treatment of intelligence and race is interesting mainly because it departs significantly from the approach taken in *The Bell Curve*. In reviewing a range of cross-cultural studies of intelligence and academic achievement, Sowell rejects a genetic basis for observed group differences. Arguing that group differences in tests of IQ and scholastic achievement represent real differences in cultural attainment (and are not just artifacts of cultural bias in the testing instruments themselves), Sowell opposes any relaxation in admission standards to achieve a more balanced representation of ethnic groups in, for example, higher education. Sowell's discussion of slavery focuses on comparisons between different cases to demonstrate that the enslavement of black Africans by Europeans and Americans was neither historically unprecedented in terms of magnitude or brutality, nor a causal factor in current problems experienced by African Americans, such as breakdown of the family or unemployment.

In general, Sowell's attempt to build a case for the independent role played by culture works better in some areas than in others. The effort to expand the examination of race/ethnicity beyond one national setting is laudable, and the coverage of a literature drawn from several disciplines is impressive. But the absence of a coherent (much less, rigorous) comparative methodology renders the analysis less compelling than it might otherwise be. Sowell's approach is to adduce various examples drawn from one or more nations, cultures, or time periods in support of his argument. This approach is the weakest of the comparative methodologies, since it does not lend itself to negative findings. Nowhere does Sowell acknowledge the extensive comparative methods literature in "so-called 'social science'," nor does he provide a rationale for his selection of specific cases (i.e., a sampling design, however rudimentary). This ad hoc treatment of empirical evidence will not trouble those predisposed to agree with Sowell's blend of social conservative and laissez-faire economics ideology. But the book's lack of methodological rigor will, I suspect, make it easy for readers holding opposing political viewpoints to discount Sowell's conclusions. This is unfortunate, since the comparative perspective undoubtedly holds important lessons for understanding racial/ethnic inequality in the United States.

Less important than the fundamental question of methodology—but worth mentioning—are several other problematic features of the book. First, in making the case for the impact of culture, Sowell often fails to identify specifically the source of opposing arguments. Instead, he typically attributes opposing points of view to generic "social science." Second, at numerous points in the discussion Sowell introduces illustrations or quotations to bolster his argument, which, on closer examination of the notes, turn out to come from his own previous publications. Although there is nothing inherently wrong with self-citation or even self-quotation, the author owes it to the reader to make it clear in the text (not buried in an end note) precisely who is being quoted or cited. In general, the editing of the book is seriously substandard. Examples, phrases, and sometimes entire sentences are repeated verbatim in the text within the space of a few pages (e.g., pp. 159 & 172; 192, 201 & 207; and 204–5). Whether due to careless word processing or the piecing together of text from previously published papers, these annoying repetitions should have been caught and corrected in the editorial process.

University of New Mexico RICHARD M. COUGHLIN

National Identity and Ethnicity in Russia and the New States of Eurasia. Edited by Roman Szporluk. Armonk, NY: Sharpe, 1994. 328p. $19.95.

This collection of conference papers is the second in a series of 10 volumes planned by the Russian Littoral Project, sponsored jointly by the University of Maryland at College Park and by the Paul H. Nitze School of Advanced International Studies of the Johns Hopkins University; the ambitious project's directors are political scientists Karen Dawisha and Bruce Parrott. The essays contained in the volume under review represent updated and revised versions of papers presented at three conferences held during May and June of 1993. As the book's title indicates, the constituent essays focus on questions of national identity and ethnicity in the *newly independent states*.

The authors are, in the great majority, distinguished specialists who are well informed about their subject areas. Most take an "area studies" approach to their topics, while only a few evidence an interest in questions of theory.

One striking omission is the editors' failure (with the exception of a single piece concerning the Abkhazian conflict in Georgia) to include coverage of developments in the Transcaucasus. One may speculate that there was a problem with the papers presented on this area of the former USSR at the three May–June 1993 conferences.

In his stimulating introduction to the volume, historian Roman Szporluk ranges perceptively over a host of difficult questions connected with the collapse of the USSR in late 1991—for example, the awkward legacy of Soviet nationalities policies; the vexed issue of Russia's treatment of its own minorities; and the key issue of Russian-Ukrainian relations, which, as Szporluk points out, involve Ukraine's emerging national identity as much as Russia's. On the question of Russia's relations with the other newly independent states, Szporluk rightly concludes, "It is too early to tell whether neoimperialism will prevail over the nonimperial nation-building model in Russia" (p. 16).

Elizabeth Teague's "Center-Periphery Relations in Russia" is one of the best treatments that I have seen of this thorny, multifaceted problem. My only regret is that her essay had to break off in September 1993 and was therefore unable to cover the final language on center-periphery relations contained in the December 1993 Russian Constitution.

Nikolai Rudensky's essay on the fate of Russian minorities in the successor states is a remarkably balanced and dispassionate analysis by a Russian author (Rudensky is a researcher at the Institute for the Economy in Transition in Moscow) of a subject that has now emerged as a major Russian foreign policy issue. Rudensky makes a helpful distinction between the terms *Russian-speaking population* and *linguistic Russians*. The former term, widely employed in today's Russia, is, he correctly notes, useless, because it includes two-thirds of the population of the former Soviet Union. The term *linguistic Russians*, on the other hand, denotes the 11.2 million non-Russians living in the successor states, who speak Russian natively. (Most are of Ukrainian and Belarusian ancestry, and live chiefly in Ukraine, Belarus', and Kazakhstan.) Even this population group, Rudensky aptly cautions, should not be reflexively viewed "as an integral part of the Russian diaspora" (pp. 59–60).

Rudensky is also forthcoming on the question of why there are, in fact, so many ethnic Russians living today in the so-called "Near Abroad." In addition to promoting industrialization in the non-Russian periphery, the Soviet regime, he writes, also pursued a conscious goal of "internationalization"; that is, it fostered the Russification of the borderlands through a sponsoring of mass migration to those regions.

Gregory Guroff and Alexander Guroff's "Paradox of Russian National Identity" is, to my mind, the most flawed contribution to the volume—which is unfortunate, because the topic is an important one. In discussing contemporary Russian's sense of national identity, the authors ought to have consulted the extensive polling data on that subject gathered by the Russian Center for Public Opinion Research and other polling organizations in Russia. The findings of such polls have been appearing in both the scholarly and popular Russian press since 1989. While the Guroffs do make brief reference to two public opinion surveys, they obviously prefer to consult their own hearts and to "ask friends, family, colleagues, and acquaintances what it means to be Russian" (p. 85). Not surprisingly, their essay is long on subjectivism and impressionism and short on data.

While the Guroffs rightly underline the importance of a knowledge of Russian Orthodoxy for an understanding of Russian historical development, they betray a lack of familiarity with recent Western scholarly literature on this subject. For example, their discussion of the well-known Iosif of Volokolamsk–Nil Sorskii dispute overlooks pertinent new information on that episode contained in Yale historian Paul Bushkovitch's pathbreaking study, *Religion and Society in Russia: The Sixteenth and Seventeenth Centuries* (1992).

The remaining essays in the volume under review focus upon conditions in the former minority union republics. This is not my specialty, but I found Ilya Prizel's analysis of present-day Ukraine and Jan Zaprudnik's examination of contemporary Belarus' to be informative and suggestive, while Algimantas Prazauskas' discussion of Ukraine, Belarus', Moldova, and the Baltic states suffered from having embraced a vast topic in a short essay. Walter Clemens, Jr., attempts to apply "the theory of fitness" to Baltic identities in the 1990s. The three essays on Central Asia—by Martha Olcott, Robert J. Kaiser, and James Critchlow—struck me as informative, sensible, and balanced presentations. Gueorgui Otyrba's "War in Abkhazia" is a useful examination of one of the most vicious ethnic wars of the post-Soviet period; the essay also sheds useful light on the situation in neighboring Chechnya, an autonomous republic that, of course, sparked a major war in late 1994.

Hoover Institution JOHN B. DUNLOP

Islamic Society and State Power in Senegal: Disciples and Citizens in Fatick. By Leonardo A. Villalón. New York: Cambridge University Press, 1995. 338p. $59.95.

In *Islamic Society and State Power in Senegal*, Leonardo Villalón makes an excellent contribution to what has become, in recent years, the dominant issue in African political studies—the relationship between state and society. While the book is clearly grounded in the experience of Senegal, it offers theoretical formulations that are relevant not only in Africa but in other developing societies.

Villalón's main thesis is that unlike most other African states, Senegal has enjoyed a relatively democratic and open political system since gaining its independence in 1960. He argues convincingly that the source of Senegalese "exceptionalism" stems from the capacity of a vigorous and organized civil society to challenge the hegemonic and predatory pretensions of the state; but it is civil society with a difference. According to Villalón, Senegalese exceptionalism does not rest on the modern, individualistic, bourgeois and secular civil society that many political scientists find necessary for the flowering of democracy. Instead, it is founded on an Islamic civil society comprising the religious institutions of Senegal's Sufi Muslim orders. As he succinctly puts it, "Religious structures and ideology have facilitated and encouraged a system of social organization outside the state and a range of possible responses to the actions of that state which have served as an effective counterbalance to its weight" (p. 15). In this vein, Villalón approaches civil society as a social space in which individuals and collectivities find and define themselves as external and opposed to the state.

Externality and opposition to the state do not, however, imply total disengagement from the state. Rather, they entail mediations and tensions, which, when properly managed, lead to mutually beneficial compromises. In fact, it is precisely when groups and individuals have shown that they can survive successfully outside the realm of the state that they can engage the state and obtain a better deal from it. In his careful study of the relationships between religious authorities (*marabout*s citizen-disciples (*taalibe*s), and the state, Villalón argues that a *marabout*'s initial popularity and ability to attract a following corresponds to his capacity to distance himself from the reach of the state. Once that capacity is well established, however, he can accept state assistance for his community without fearing a loss of popular legitimacy. In fact, engagement with the state is inevitable if the *marabout* is to institutionalize and consolidate his hold over a growing number of *taalibe*s. In his analysis of the development of a maraboutic movement in Fatick headed by the Serer Ma Ansou Niang, Villalón convincingly shows that whereas the movement's early growth stemmed from its distance from the state, its capacity to expand further will depend on some degree of engagement with the state. The very inevitability of engagement indicates the economic "limits of a radical isolationism" (p. 261).

Just as *marabout*s and *taalibe*s must tolerate a relative intrusion of the government into their private preserve in order to ensure their material well-being, state bureaucrats and politicians cannot ignore the maraboutic communities if they want to enhance the reach of the public realm. Sacred power and temporal power are intertwined. Mutually engaged, they constrain, mediate, and facilitate the growth of their respective domains.

In spite of being intertwined, politics and religion in Senegal have nonetheless retained their autonomy. This is not to say that maraboutic power has no political effects; for as Villalón clearly demonstrates, it does. There have been occasions when *marabout*s themselves have directly intervened in the political arena. For instance, in 1988 Abdou Lahatte, the *xalifa*-general of the Mourides pronounced a *ndigal*—a religious order—commanding all his disciples to vote for the reelection of President Abdou Diouf. This open identification of a major *marabout* with the political powers that be is more the exception than the rule; since the death of Abdou Lahatte in 1989, the Mourides have opted for a more restrained position and their new *xalifa*-general did not pronounce a *ndigal* during the 1993 elections. Generally, the most powerful *marabout*s have sought to avoid public entanglement in partisan politics even in times of acute crisis. Villalón points out that in the aftermath of the very controversial 1988 elections, when a tense struggle for power pitted the opposition led by Abdoulaye Wade against President Diouf for more than two years, the *marabout*s "maintained themselves carefully above the fray" (p. 262). Villalón thus persuasively concludes that the "non-centrality of Islam to the severe political battles which Senegal has experienced in recent years . . . indicates that the political importance of Islam in Senegal has been concentrated in the domain of state-society relations and not in the struggle for control of the state" (p. 262).

For Villalón, the "relative goodness" of Senegal's government derives from the character of state-society relations in which sacred and temporal power "engage on a relatively even footing" (pp. 259–60). Essential to this balance of power is the *daaira*, a specifically Senegalese institution principally found in urban areas. Villalón defines it as the "unquestioned unit of religious allegiance for groups of a marabout's disciples in a given locale" (p. 151). Given the *daaira*s' size and capacity to generate a sense of community, they can become potent social forces that can effectively curb the predatory reach of the state. Moreover, these *daaira*s' function as quasi-parallel forms of government doted not only with a spiritual independence but also with their own internal systems of taxation and fundraising. Maraboutic communities (especially the more successful ones, e.g., the Mourides and Tijaniyyas) thus have some real economic autonomy.

A key beneficiary of this autonomy is the *marabout*, who by virtue of his religious authority, extracts some of the fruits of his *taalibe*s' labor. Villalón argues that such material extractions even if they are symptomatic of "unjust inequalities" entail "an important degree of reciprocity" between *marabout*s and *taalibe*s (pp. 187, 192). The author shows that there are clear limits to the *taalibes'* allegiance and thus to the exploitative practices of the *marabout*. But if there are limits to exploitation, they are just that and do not negate the very real inequities of the maraboutic system. Villalón may have thus exaggerated the reciprocal features of the *marabouts-taalibes* relationships and in the process neglected the democratic impairments afflicting Senegal's Islamic civil society.

So while Villalón does a superb job demonstrating how Senegalese exceptionalism is rooted in the capacity of the Sufi orders "for alternatively challenging the state or limiting its influence by isolating social groups from state initiatives" (p. 260), he fails to explore the possibility that the difficulties and authoritarian reflexes characterizing this exceptionalism may well be rooted in the submissive and exploitative relations embedded in Islamic civil society itself. Even if Villalón may have underestimated the undemocratic nature of the Sufi orders, he has written an important and well-researched book. *Islamic Society and State Power in Senegal* is a must for all scholars interested in Senegal and African politics. It deals intelligently and creatively with the fundamental relationships structuring the interaction between state and civil society, politics and religion, and leaders and followers. It is unfortunate that Cambridge University Press has priced the book out of reach of a large audience of students and scholars; one can only hope that *Islamic Society and State Power in Senegal* will soon find its way into an affordable paperback edition.

University of Virginia ROBERT FATTON, JR.

INTERNATIONAL RELATIONS

The United States and Israel: The Limits of the Special Relationship. By Abraham Ben-Zvi. New York: Columbia University Press, 1994. 312p. $35.00

Abraham Ben-Zvi's *The United States and Israel* is the first major effort to create a theoretical and comparative approach to the relationship between the United States and Israel. Most of the previous literature has been written either by journalists or by political scientists employing a nontheoretical comparative approach. What, then, is Ben-Zvi's theoretical framework? His conceptual and analytical framework deals with two main strategies of international relations theory: deterrence and coercive diplomacy. The author concludes that America's attempts to constrain Israel through the use of these methods were not successful.

To illustrate the American coercive and deterrence policy, Ben-Zvi uses several case studies. From the water crisis (1953) to the Sinai crisis (1956–57), to Lebanon (1982), to the Intifada (1987), to the Persian Gulf (1991), all crises are clearly summarized and succinctly analyzed. Each case study involves the proposition that on the whole, the United States, regardless of how much influence it has over its smaller ally, has not succeeded to deter Israel or coerce it when it came to its most precious security and national goals. This part of the book is quite innovative and significant.

However, the main problem with the book stems from the jungle of conceptual and analytical models introduced without explanation, which are, in my view, artificially applied to the case studies. Ben-Zvi does not seem to employ one specific model of international relation but a plethora of them, engaging respectable "notions from the fields of bargaining and crisis theory" from Holsti to Smoke to Alexander (p. 2). Either the model should suggest an explanation and a pattern of behavior that is consistent throughout the 40 years of relationships examined and thus explains and enlightens us as to the nature and structure of the crises or the different crises should suggest a theoretical all-inclusive explanation. Neither is demonstrated. The models and concepts do not add much to Ben-Zvi's interesting and innovative explanations of each case study and crisis. In fact, they are unrelated to the nature and structure of the crises.

I find chapter 9, a postscript on the 1991 Gulf War and Israeli-American relations, the best and most interesting of the chapters. This and the chapter on the 1956–57 Sinai crisis are both based on coercive diplomacy, which certainly is a useful explanatory tool regarding the relations between nations. In other words, the author tells us that American national interests dictate a posture of coercion, which, in the case of Secretary of State Baker, went further than coercion—a subtle effort to topple the Shamir government. The two cases, according to the author, are where coercive diplomacy prevailed over "the Special Relationship," which has been the consistent, long-term policy toward Israel. We hardly need complex models to explain the diplomacy of great powers as they relate to smaller states. An extreme case, of course, is the Munich Agreement of 1938, where two great powers, Britain and France, imposed upon Czechoslovakia an arrangement in conformity with their national interests, not with the Czechs'. In fact, a comparative analysis of President Kennedy's reneging on the U.S.–British missile deal with Prime Minister Harold MacMillan would be useful as another case of Special Relationships, where perceived national interests violated that relationship. Above all, bargaining theory (Ben-Zvi's preferred tool of analysis) explains the *process* and *behavior* of actors in and out of crisis but not the purposes, motives, goals, and aspirations, which, in my view, represents bargaining theory's serious flaw. A social scientist who wants to construct a model of strategic behavior and bargaining between allies and/or rivals must find out what motivating factors determine the bargaining, what the strategic possibilities are, what the consequences of various combinations of strategic choices are, and what the actors preferences over consequences are. The case studies could furnish some answers.

If one were to consider just the major part of the book, which deals with crises, I would have no difficulty in maintaining that the author has succeeded in analyzing and explaining each and bringing them together under the umbrella of coercion and deterrence theories. However, early in the book, Ben-Zvi tells us that these models and concepts will serve as his principle analytical tools, although "in a revised form that takes into account the specific nature of the American-Israeli dyad" (p. 3). What is the analytical content of the concepts? Persuasion, deterrence, and coercive diplomacy, "the premise of which is modified and constrained by the special feature of this bilateral setting, guided and directed the architects of American Middle East policy in their recurrent efforts to influence Israel's behavior" (ibid.). So what is new? Is it not the function of diplomacy to persuade, to deter, to coerce, to influence, to bring upon concessions? Why do we need an elaborate, middle-echelon model and concepts to explain the diplomatic and political relationships between Israel and the United States? We know that deterrence, coercion, and persuasion all are essential parts of relations between nations from days immemorial. In fact, the use of a number of theoretical models makes parts of the book cumbersome. For instance, in explaining the "superpower crisis," (i.e. the October 1977 event that led to the Egyptian-Israeli accommodation and peace treaty, Ben-Zvi tells us that President Carter's outlook, in its various manifestations, "approximates Holsti's 'Post–Cold War Internationalism', Yergin's 'Yalta axioms', Zimmerman's 'organismic or cybernetic conception', Dallin and Lapidus's 'interactionist approach', Schneider's 'liberal internationalism', and Wittkopf's 'cooperative internationalism' " (p. 104). But what in the world is the connection between all these concepts? Yergin's "Yalta axioms" has to do with his perception of an antagonistic state department to the Bolshevik revolution; so does Dallin's. We would need another chapter just to explain Holsti's "Post–Cold War Internationalism" or Zimmerman's "organismic or cybernetic conception." Why not write a book on theories of international relations, rather than appropriate the headlines of these theories as an introduction to his case studies? If the author wants to propose a theory of international relations based upon Israel's case studies, he certainly does not achieve it. This must be his purpose—Why otherwise include 27 pages (out of 211) of a complex theoretical framework?

The "theoretical framework" suggested by the author is incomplete and inconclusive. The theoretical framework—not sufficiently explained, elaborated, or tested—did not do what the case studies did, that is, demonstrate the very constraints and the consequences.

I am very sympathetic to any scholar's attempt to create a new or another—theory of international relations; and it is commendable that Ben-Zvi tried (although, in my view, unsuccessfully) to establish the link between theory and the case studies. One would expect a conclusion bringing together the theories as they explain the behavior of the actors in the crises. But unfortunately, there is no such conclusion. We are speaking of two different books in one, neither complete. Unquestionably, a book that will synthesize a theoretical and comparative framework for the analysis of international relations must and will gain great reputation and considerable influence. I do admire Ben-Zvi's great effort to introduce for the first time the Israeli–American relationships on a higher level than simple journalism or unrelated narrative, and for this reason I recommend the use of it for students of Israeli–American relationships. This book is a useful one for political science, although flawed in its purpose.

American University AMOS PERLMUTTER

European Community Decision Making: Models, Applications, and Comparisons. Edited by Bruce Bueno de Mesquita and Frans N. Stokman. New Haven: Yale University Press, 1994. 259p. $30.00.

This book lies at the intersection of three research areas: applied modeling, political forecasting, and comparative politics. Its subject matter (or, more accurately, its laboratory), given its foundation in applied modeling, is decision making within the Council of Ministers of the European Community.

Although the book includes nine contributors, it is dominated by the arguments of its two coeditors, who each lay out in separate chapters models of decision-making processes. These models are subsequently tested in a series of coauthored chapters, and the collection as a whole is introduced and concluded by chapters written by comparativists and specialists in American and international politics. First and foremost, this is an exercise in applied modeling. The book turns on the elaboration and comparison of a set of expected-utility models and a set of exchange (or logrolling) models of political decision making, understood as noncooperative and cooperative games, respectively. The models are tested using data on decisions taken in the Council in four policy fields: permissible levels of radioactive contamination in foodstuffs, emission levels for vehicular exhaust, the liberalization of civil aviation, and the development of the community banking system. These, it is argued, represent a broad cross-section of the Community policy domain (p. 34). The data requirements are minimal. They include estimates of actor capabilities, the salience for an actor of a decision, and the actor's policy (most preferred) position on a decision. Some of these data are generated through the use of expert evaluations. For the most part, the actors are defined as the member-states participating during the negotiations in the Council. The models amount to algorithms that translate these data into predictions regarding the final decision reached by the Council of Ministers. Both sets of models do well. An expected-utility model (the *conflict model*, by the end of the book) that builds in strategic choice does the best of all. Although there is substantial correlation between their predictive results, their "error terms" are not as strongly correlated and the hope is that those conditions that determine when each model does best can be established in further work.

This is a high-quality book. Its contributors handle a series of difficult theoretical, methodological, and empirical problems with great skill and care. There are many more detailed points of interest (e.g., the fascinating discussion of "controlling strategies," pp. 101–2, 153–60). It will no doubt be of interest to applied modelers, forecasters, and students of the European Community, probably in that order. However, the models strike me as somewhat idiosyncratic. Are there more effective ways to model strategic choice than the theoretical tack taken by Bueno de Mesquita? Can the exchange model be restated in the formal language of expected utility? In other words, these applied models seem to need stronger backward linkages to theory. I do not mean that the authors do not draw on theory in ways that they are careful to specify. Bueno de Mesquita is careful to show the importance of the median voter theorem and work in international relations theory on escalation for his expected utility model. Stokman set his work on exchange in a broader context. They are certainly aware of the concessions and adjustments that are needed for the purposes of testing, problems that pure theory can avoid. Indeed, they argue in their wrapup that the assumptions they make for purposes of tractability may be violated in reality and produce predictive error (p. 226). there is still the issue of linkages to theory. it is surprising to see so little use made of game theory, for example, despite the occasional reference to game-theoretic bargaining solutions (e.g., pp. 97–111).

As this work proceeds, it should also be possible to build stronger linkages to the community of scholars who work on the European Community, linkages that extend deeper than the use of expert knowledge to generate such thin data as estimates of the salience of issues. For better or for worse (I think for the better), students of the Community are much more interested in process than prediction. The models of this book do have implications for process. This is clear from the way that Bueno de Mesquita and Organski tell an analytical story using his model of strategic interaction and from the chapter on banking by Kugler and Williams. But more can be done here, it seems to me—although as it is pursued, the quasi-independent status of "applied modeling" may be threatened, since comparativists will no longer be mere adjuncts qua experts. As further work on the Community is pursued, their arguments about the Council can also be set within a broader picture of European Community decision making that includes its other institutional actors. One goal here might be to build models of policy process—also rooted in rational choice theory—that begin explicitly from the institutional characteristics of the Community. These might be different in interesting ways from the models elaborated here, and they might even solve the problem of realism that the authors associate with the predictive errors of their models. Further, if these models begin from institutional features of the Community, those scholars who elaborate them are, in a meaningful sense, already students of the European Community, rather than applied modelers; and linkages to the broader group of scholars who work on the Community is then easier.

McGill University HUDSON MEADWELL

The State in Transition: Reimagining Political Space. Edited by Joseph A. Camilleri, Anthony P. Jarvis, and Albert J. Paolini. Boulder: Lynne Reinner, 1995. 245p. $45.00.

In a characteristically thought-provoking essay, R. B. J. Walker asks, near the beginning of this book, "Who now really believes there is a theory of the state?" (p. 24). The very fact that this question can now be raised with an international relations audience in mind suggests that there has been something of a sea-change in the field. Paradoxically, although the state has generally been regarded as the dominant actor in the contemporary international system, there has, until recently, been remarkably little interest in developing any kind of theory of the state from an international relations perspective. The state has been unproblematically defined, in textbook fashion, by the presence of a people, a territory, and a government. This volume of essays illustrates very effectively that such a formulation of the state begs enormous questions and that any attempt to think seriously about the state must embrace an international or global dimension. Too often, this dimension has been pushed to one side in attempts to theorize about the state. The 13 chapters that make up this volume are the product of a conference held at La Trobe University in Melbourne, Australia; and the book is the latest addition to the Critical perspectives on World Politics series, edited by Walker.

The book works on the premise that the state has entered a transitional phase that is likely, the editors argue, to result in a reconfiguration of political space. The premise is explored from two different angles. The first focuses on the identity of the people who occupy the political space contained by the state. The simple equation traditionally drawn between the state and the nation, with new states being given the task of nation building, has started to look a good deal more complex since the demise of the Soviet Union. The chapters by Sanjay Seth and Irmline Veit-Brause convincingly demonstrate that while nationalism has provided the modern answer to the problems raised when fixed territorial boundaries have been mapped onto the inherrently elastic borders generated by group identity, it has not eliminated or solved these problems. There is a constant need for redefinition, and John Fitzgerald provides a fascinating case study of the way that the changing national flags in China have provided symbols for the attempts by successive governments to rearticulate or reimagine the composition of the Chinese people.

The processes linked to globalization constitute a second factor, which is seen by the editors to be precipitating a reconfiguration of political space. Globalization is seen to challenge the assumption that political space is bounded by the state. This assumption, it is argued, has represented a defining feature of modernity itself, because it has been presupposed, in the past, that global political space is necessarily parcelled out among states. As Jarvis and Paolini put it in the opening chapter, there is "no empty space between states." Modernity is seen to offer "a vision of the global political space as constituted entirely and exclusively by states" (p. 4). From this perspective then, the state becomes the only possible site for politics. But globalization is seen to be challenging the hegemony of the state, because, as Anthony Elliott points out, we are now confronting risks that transcend the boundaries of time and space. These risks (evidenced by e.g., the dangers posed by nuclear war, global warming, and world economic recession) are seen to be moving us into an era where apocalyptic risk taking has become a habitual fact of life. Social theorists, such as Beck and Giddens, have argued that these dangerous products of globalization have generated a very substantial increase in the scope for self-monitoring, or what they call *reflexivity*. Elliott, however, advances some trenchant criticisms of this position, suggesting that monitoring by functional specialists can lead to the reproduction of irrational policies. His analysis of the West's response to the conflict in Bosnia provides a telling illustration of this point. He advocates the need for critical or creative reflexivity that moves beyond self-monitoring to the point where there is "an openness to difference and otherness," a willingness to turn knowledge 'back on itself,' and an acknowledgement of the need to examine the "contradictions of the social order" (p. 69). Such processes necessarily require a view of political space that transcends the boundaries of the state.

The contributors to this book all take the state seriously, and this represents a substantial advance on the discussion in the 1970s about interdependence and transnationalism when the role of the state in the international system was questioned without being subjected to critical interrogation. But there are weaknesses in the approach adopted by the editors. First, they fail to make provision for the idea of *res communis*. Deeply entrenched in the modern state system has been the acceptance that there are areas of the globe to which there is open access. Sovereignty has never been the only principle that has governed the organization of political space. Once consequence of globalization has been the growing recognition, moreover, that the principle of *res ocmmunis* should be extended to embrace the idea of common heritage, with the resources of the sea, for example, being developed for the benefit of everyone, not just those who have access to these resources. States, however, have resisted this development, successfully extending their territorial control vertically up into air space, and horizontally out into the sea, beyond the traditional three-mile limit. These developments are embodied in two international regimes that along with others, have had a substantial impact on the organization of political space. The significance of regimes, however, is totally ignored by all the contributors, although their existence demonstrates that when thinking about political space, states have been rather more imaginative than is acknowledged in this volume.

The editors also fail to take sufficient account of the continuing importance of power in the configuration of political space. Only James L. Richardson directly confronts this issue. Globalization, he asserts, is not simply the product of impersonal forces. Its form has been promoted by the prevailing ideology, which he identifies as "one-sided market liberalism" (p. 149). The ideology has encouraged liberal democratic governments to set aside the goals of social equity and full employment in favor of competitiveness and efficiency. In the pursuit of these goals, policies have been deliberately introduced to erode the authority of the state, and this has diminished its capacity to respond to the many challenges posed by globalization. As Richardson sees it, therefore, liberalism serves the interests of the strong at the expense of the weak and, at the end of the day, legitimizes a global configuration of political space that reinforces the established structures of inequality. Whether or not one agrees with Richardson's assessment of the universal decline in state power and the detrimental effects that have flowed from liberal ideology, it remains the case that it is necessary to make provision for the impact of power in any attempt to identify and understand the reconfiguration of global political space.

University of Bristol RICHARD LITTLE

Power Rules: The Evolution of NATO's Conventional Force Posture. By John S. Duffield. Stanford University Press, 1995. 386p. $49.50

John Duffield has written a fine history of the evolution of NATO's conventional force posture from 1949 until the end of the Cold War in 1989. In contrast to the role of nuclear forces in NATO strategy, the role of conventional forces has long been neglected in the voluminous writings on the Atlantic Alliance. Duffield not only fills this gap, but also adds to the more general historical record on the development of NATO strategy.

Relying almost entirely on primary sources, Duffield details how NATO's conventional force posture evolved from 1949 until the Cold War's end, paying particular attention both to NATO's conception of the role of conventional forces in the Alliance's strategy and the actual conventional capabilities it deployed in the Central Region of Europe. The bulk of the book focuses on developments in the early years, including the frequent changes in strategy and force levels in the late 1940s and early 1950s and the emergence of a more stable pattern in strategy and capabilities from the early 1960s onward. Here, Duffield ably recounts the history of NATO's sudden transformation from a political into a military alliance following the onset of the Korean war, the adoption and subsequent abandonment of the ambitious conventional force goals during the Lisbon meeting in 1952, the struggle over German rearmament, and the changes in NATO strategy from MC 48 to MC 14/2 and, finally, to MC 14/3. The prose is lucid, the story is detailed and richly told, and the effort is comprehensive in scope. There are a few new details, though little to change the accepted historical record.

Aside from providing a historical overview of the evolution of NATO's conventional force posture, Duffield's second purpose in writing this book is analytical. Specifically, Duffield uses three different theoretical perspectives to help explain how and why the conventional force posture evolved in the manner it did. These perspectives (balance-of-power, intraalliance, and institutional) Duffield rightly insists, offer complementary, rather than competing, explanations of this evolution. Thus, the balance-of-power perspective helps explain the broad changes in NATO strategy and conventional forces, especially in the early years, and set the limits within which the evolution of NATO's conventional force posture occurred. The specific direction of this evolution, however, requires additional explanation. Thus, Duffield suggests that the intraalliance perspective is necessary to explain the growing ambiguity of NATO strategy over time, because this resulted from the increasing divergence of strategic preferences among the major members. Finally, Duffield argues that the development of a series of norms and rule, as predicted by the institutional perspective, helps to explain why NATO's conventional force posture was remarkably stable from the early 1960s onward.

Based on this empirical evidence, Duffield suggests that the evolution of alliance behavior generally can best be explained by the development of institutional norms and rules within a relatively stable external environment. Balance-of-power considerations are dominant only when institutions are unformed or (as in the late 1980s) when the international environment undergoes rapid and massive upheaval. In short, the balance-of-power perspective will determine the external environment within which alliances operate and, together with the intraalliance dynamics, set the parameters for alliance behavior. Within these parameters, however, it is the internal characteristics of the institution that is most likely to be determinant.

Though unobjectionable, this analytical explanation of the evolution of NATO's conventional force posture has some important shortcomings. There is, of course, the inherent problem of relying on a single case, which limits the analytical generalizations that can be drawn from Duffield's empirical findings. But even as an explanation of the particular case at hand, there are some problems. Perhaps the most important of these is that Duffield relies on a very narrow definition of conventional capabilities, focusing on the number of divisions and brigades deployed in the Central Region of Europe. In so doing, he ignores other aspects (e.g., the quality of equipment, training, morale, unit cohesion, readiness) that determine the military effectiveness of the forces deployed in the field. While recognizing their importance, Duffield maintains that these qualitative factors are difficult to measure, and he therefore excludes them from consideration. But this misses a critical factor of change, especially during the time when, according to Duffield, NATO forces remained highly stable. For while the number of brigades in Europe changed little from the early 1960s to the late 1980s, their combat effectiveness surely did. For decades, the United States and NATO expended vast resources to upgrade the technological sophistication of the military forces deployed in Europe in the belief that superior quality could compensate for inferior quantity. Therefore, to say that NATO's conventional capability remained stable from the mid-1960s onward is true only in a quantitative sense but not in any sense that is militarily relevant.

Notwithstanding this analytical shortcoming and the questions it raises about the overall validity of Duffield's theoretical argument, the book is a major contribution to the growing historical research on the Atlantic Alliance. It is sure to remain the standard reference work on NATO's conventional force posture during the Cold War for quite some time.

University of Maryland IVO H. DAALDER

Israel's Nuclear Dilemma. By Yair Evron. Ithaca: Cornell University Press, 1994, 327p. $39.95.

As Professor of political science and a fellow at the Jaffee Center for Strategic Studies at Tel Aviv University, Yair Evron is perhaps Israel's top academic strategist. His writings include deterrence of nuclear war, conventional warfare, and international terrorism.

Israel's Nuclear Dilemma makes an outstanding contribution to the strategic studies literature in general and to the literature on the Middle East in particular. It discusses Israel's conventional deterrence posture, the limited relevance of its presumed nuclear capabilities, and the nuclear status of Middle Eastern states. Because the book is about Israel, it does a much better job examining its nuclear status than that of other states in the region. *Israel's Nuclear Dilemma* raises issues concerning the merits of nuclear ambiguity, proliferation, and the Nonproliferation (of Nuclear Weapons) Treaty (NPT).

With regard to the first issue, nuclear ambiguity, Evron discusses whether Israel should continue its ambiguous nuclear posture. He takes issue with those, such as Shai Feldman his colleague at the Jaffee Center, who suggest that Israeli nuclear ambiguity be reduced in favor of an explicit declaratory nuclear policy. Evron's analysis indicates that nuclear ambiguity continues to yield positive outcomes, while explicitness would have negative results, especially for the Middle East.

The Gulf Crisis and Gulf War raised the paradox of a policy of nuclear ambiguity. On the one hand, if Israel develops an explicit declaratory nuclear policy, it provides incentives for Israel's adversaries to develop nuclear weapons capabilities. On the other hand, if Israel is too ambiguous regarding the nature of moves that would trigger nuclear retaliation and the type of response, that ambiguity could degrade the implicit threat of a presumed nuclear capability. So with respect to the Gulf Crisis and Gulf War, Israel's policy of ambiguity resulted in the lack of official nuclear threats. Evron rejects the claim that Israeli nuclear threats played an important role in deterring Iraqi use of chemical weapons against Israel during the crisis and war. For Israel to have adopted a nuclear deterrence policy, it should have been less ambiguous both about what Iraqi moves would trigger response and the nature of that retaliation, according to Evron.

While officially maintaining an ambiguous posture, Israel has continued to develop its nuclear capabilities. Despite enhancing its capabilities, however, Evron demonstrates in several historical cases that Israel has not officially made use of nuclear deterrence. In addition to the Gulf Crisis and Gulf War, Evron concludes that nuclear weapons were of no relevance in the 1967 War, the 1969–70 War of Attrition, or the 1973 War.

A second issue the book poses is whether the spread of nuclear weapons has mainly positive or principally negative consequences. Evron belongs to the "more is worse" school of thought, along with such scholars as Scott Sagan of Stanford and Gerald Steinberg of Bar Ilan University in Israel. In contrast, there is the "more is better" school, which includes scholars like Kenneth Waltz of Berkeley and Shai Feldman. Indeed Sagan and Waltz have combined their contrasting views in a book about the merits of nuclear proliferation entitled, *The Spread of Nuclear Weapons: A Debate* (1995).

Both Sagan and Evron argue the realist viewpoint: nuclear proliferation would make the world less stable because new nuclear states lack necessary organizational structures to ensure safe and rational control of their weapons. For the Middle East, Evron concludes that proliferation would greatly increase the likelihood of war. In contrast to Evron, Feldman holds that nuclear proliferation would have a stabilizing effect on interstate relations in the Middle East.

It seems as if Evron wins his debate with Feldman. During the Cold War, assured destruction resulted in a nuclear-based stable deterrence system between the superpowers. Deterrence, however, was not the order of the day for the Middle East region. As the superpowers moved from crisis to crisis without warfare in the global village, Israel and its neighbors lurched from crisis to crisis and from war to war in their tough neighborhood. So while nuclear weapons facilitated stable deterrence at the global level, mutual possession would have contributed to the escalation of crises and subsequent destructiveness of local wars. It is difficult to effect a secure second strike applicable to regional states pursuing ethnic and religious goals and possessing high intensity conventional weapons.

Another difference between the global and regional levels concerns the magnitude of available weaponry. Nuclear armaments are not very effective in war fighting. But these high-yield weapons are suitable for deterrence of an attack. In contrast to Evron's view of the lack of relevance of Israel's nuclear capabilities, there is the position of policymakers. Because of the Arab advantage in manpower and tanks, the dominant view among Israeli policymakers is that Arab uncertainty about Israel's possession of nuclear weapons is an effective deterrent. But with nuclear weapons on both sides, stable deterrence is not as likely as it is in the global arena. The bottom line is that success of nuclear deterrence at the global level does not imply that mutual possession of nuclear weapons by local powers would lead to stable deterrence in the Middle East region.

A third issue raised by Israel's Nuclear Dilemma concerns the NPT. Because of Israel's policy of studied ambiguity, it declines to sign the NPT. Signing, either as a nuclear or nonnuclear weapons state, would sacrifice the benefits of ambiguity and carry with it the risk of offering additional incentives to Israel's adversaries to develop nuclear weapons capabilities. Its position is that until there is a general peace in the Middle East with arms control and verification, it will not sign the treaty.

Israel, India, and Pakistan are three countries thought to have nuclear weapons or the capacity to develop them on short notice. During April and May 1995, the United Nations held a conference on the extension of the NPT. Fourteen Arab nations had submitted a resolution intended to draw attention to Israel's undeclared nuclear program. But on 11 May 1995, 170 nations agreed to extend the NPT in perpetuity, even without the resolution naming Israel.

By not singling Israel out for condemnation, the international community gives credence to the view that Israel's ambiguous nuclear posture should not prevent extension of the NPT. In effect, the world community may be giving implicit approval to ambiguity over a declaratory policy and thus providing support to the view laid out by Yair Evron in *Israel's Nuclear Dilemma*.

University of Michigan RAYMOND TANTER

Sunken Treasures Naval Arms Control between the Wars. By Emily O. Goldman. University Park: Pennsylvania State University Press, 1994. 352p. $45.00

Emily Goldman has written a sound historical narrative exploring the interwar experience with naval arms control. She makes an important contribution by reminding us that conventional arms control was practiced by a number of states in multilateral forums prior to the Cold War; superpower nuclear arms control, in other words, is not the only game in town. Goldman persuasively argues that the arms control strategy pursued throughout much of the Cold War (the "technical" approach) was a highly truncated version of the richer set of strategies (the "political" approach) that informed key aspects of the interwar arms control experience.

A prime purpose of the book is to ask what lessons this earlier experience has for the theory and practice of arms control in the post–Cold War era. Goldman largely succeeds in making the case that the 1920s and 1930s hold important lessons for policymakers interested in arms control today. At a theoretical level, however, the book is much weaker, falling prey to the inherent weaknesses of the inductive approach it adopts.

In policy terms, the author is right on the mark to argue that arms control is sure to produce limited results when it focuses solely on technical criteria (e.g., stabilizing force postures) at the expense of the broader political and strategic issues separating two (or more) countries. Goldman's carefully constructed case study of the interwar naval arms control process (chaps. 4–6) demonstrates that it is the coupling of political and technical elements that gives particular treaties real staying power.

In the book's last chapter, Goldman utilizes this historical record to distill a series of lessons (many of which seem quite sensible) for the practice of arms control in the post–Cold War period. Indeed, writing in 1993, she comes close to predicting what eventually came to pass with regard to Ukraine's nuclear weapons. It was only after Russia, the United States, and Ukraine jointly addressed the latter's political and strategic concerns that the Ukrainian nuclear issue was settled—quite successfully, as subsequent events indicate.

The theoretical contribution of the book is primarily limited to chapter 1, which begins with a critique of the deductive approach to arms control that constituted the Cold War "orthodoxy." This approach, Goldman correctly argues, is informed by the assumptions of neoclassical economics and conceptualizes the arms control process in very static terms. States enter a negotiation with a stable and exogenously determined set of preferences that may or may not (depending upon the degree of divergence between the two sides' interests) lead to an outcome—a particular treaty.

Goldman favors, in contrast, a more dynamic understanding of arms control where preference formation is endogenized; that is, the very *process* of negotiating agreements can reshape state interests and preferences. This is especially likely to occur if a broad-based political strategy of arms control is pursued. The book's case study of the Washington and London treaty conferences does indeed provide some evidence that state preferences shifted during the process of negotiation.

A correlation, however, is not the same as a causal argument. Goldman's theoretical approach and use of data undercut her ability to argue causation. She consciously adopts an inductive method where the "political, diplomatic, economic, and strategic assumptions that influence how states define security" are all examined (p. 21). Such an approach has strengths and weaknesses. Goldman's careful recounting of events during the interwar years provides one with a real sense of the contingent and changing nature of state preferences and the many factors influencing their formation. What she cannot do is isolate the independent causal influence of any given factor. Most notably, she fails to make a plausible case for the book's central thesis—that the process itself changed state preferences.

There are at least two problems here. For one, she does not specify how *process* is operationalized. Is it the actual negotiations in Washington and London (and elsewhere)? Or should process be conceived more broadly as the interaction between international negotiators and domestic political actors in particular countries? To talk of the "social and communicative side of the policy process" that the deductive approach to arms control ignores is to highlight the *importance* of process without *defining* it (p. 240; see also p. 25). As a result, it is not clear exactly how the "process" was reshaping state preferences.

A second, more serious, problem is that Goldman's largely atheoretical approach undercuts her own quite laudable efforts to assert that process does indeed matter. She offers no theory to explain how and under what conditions process (however defined) should reshape preferences. The result is an overdetermined outcome, with process being one of several possible factors shaping state interests (e.g., pp. 111–13, 189). A related problem is the book's failure to consider, in any systematic way, possible alternative explanations. Could a domestic politics or international structural argument, for example, explain the variance in state preferences over time?

Because the book's prime focus is not theoretical, it also fails to engage broader debates in the literature. For example, Goldman's emphasis on "strategic visions" and "shared doctrines" as determinants of state preferences (chap. 3) would have benefitted from a consideration of research on the role of ideas in international politics. More important, the book would have been strengthened by building upon other recent work seeking to restore a process-oriented element to our understanding of international politics. This includes both the state-centered research on two-level games in international negotiations (Evans's *Double-edged Diplomacy* [1993]) and the literature on the new transnationalism (Haas's *Saving the Mediterranean* [1990] and Risse-Kappen's *Bringing Transnational Relations Back In* [1995]). As written, the book leaves it to the reader to tease out connections to these broader debates and literatures.

Issues of methodology and theory building aside, one can still ask whether Goldman makes a plausible case for the importance of process. The evidence is certainly suggestive, but I was not convinced—a major problem being the quality of the data. Goldman at several points correctly notes that to be convinced by her argument, we have to remove the "black box" surrounding the arms control process. Her analysis, however, relies overwhelmingly on the existing secondary literature, much of which has little to say about the actual negotiating process. Indeed, despite an impressive listing of official and private papers in the bibliography, there are only a dozen or so references to such works in the entire manuscript.

In sum, Goldman has produced a useful, if limited, book. Its theoretical contribution is minimal; however, given the policy focus of much of the arms control literature, this is perhaps not terribly surprising. At a more practical level, the book should be of great interest to both policymakers and students of national security policy. Rescuing arms control from the narrow, technical approach of recent decades is the first step toward developing a broader, more theoretically driven agenda for future work on a topic whose importance has clearly not waned with the Cold War's passing.

University of Pittsburgh JEFF CHECKEL

Whose World Order? Uneven Globalization and the End of the Cold War. Edited by Hans-Henrik Holm and Georg Sørensen. Boulder: Westview, 1995. 246p. $55.00 cloth, $17.95 paper.

Several years have passed since the end of the Cold War. Despite expectations in some quarters that a "new world order" would soon emerge, political turmoil abounds. Although hostilities have not flared up between any of the great powers, festering ethnonationalist conflicts long overshadowed by the Soviet-American rivalry now frequently deteriorate into warfare. Other disputes, born of acute environmental scarcities and unrelenting population pressures, threaten to become violent. Sadly, the international community appears unable to quell much of this instability. From Mogadishu to Sarajevo, its efforts at peacemaking have all too often been marked by hesitation, false starts, and policy reversals.

Contrary to the vision articulated in 1991 by U.S. President George Bush, Hans-Henrik Holm and Georg Sørensen do not see a new world order on the diplomatic horizon. Nor do they believe that realist theorizing adequately explains why it has not materialized. What is needed, in their opinion, is a more specific, middle-range theory that (1) focuses on changes in the role of the state, (2) contains a richer conception of sovereignty, and (3) gives greater attention to the global political economy.

The challenge for theorists during periods of global turbulence is to differentiate what is truly new from those properties of interstate politics that have changed little since the days of Thucydides. *Whose World Order?* draws our attention to what is described as a new, ideological structure of dominance that Holm and Sørensen contend will supplement the older economic domination of center over periphery. From their perspective, the end of the Cold War has combined with the long-term process of globalization to facilitate linking the idea of liberal democracy with a theology of the market. For Holm and Sørensen, globalization is a form of accumulated social change that involves the deepening of political, economic, and cultural relations across borders. Because of its uneven intensity and geographic scope, as well as due to differences in the societal structures possessed by nation-states, they argue that we find ourselves in a world characterized by enormous regional variation. There is no overarching political order. Regardless of the rhetoric expressed by the victors in the Gulf War, the central feature of international politics today is transitional change in the patterns of hierarchy and dominance.

To explore the impact of globalization and the end of the Cold War on the prospects for order, Holm and Sørensen have assembled an interesting collection of essays written by prominent scholars from eight different parts of the world. Each of the essays addresses two questions: (1) What has changed in political, ideological, and economic terms since the end of the Cold War? and (2) What are the consequences of these changes for theorizing about international relations? By focusing on common questions, the volume avoids the problem of disjointed contributions that plagues many edited books. What is more, it offers the reader fascinating insights into how observers from other cultures perceive the structure and functioning of the current international system. In addition to theoretical chapters by Holm and Sørensen and by Robert O. Keohane, the regional coverage includes Africa (Claude Ake), Latin America (Osvaldo Sunkel), South Asia (Gowher Rizvi), China (Zhang Yunling), Russia (Vladislav M. Zubok), Pacific Asia (Takashi Inoguchi), and Europe (Michael Zürn). Unfortunately, the volume lacks a chapter on the consequences of globalization and the end of the Cold War for the Middle East.

With such a lively mix of scholars, it is not surprising that differences of opinion surface. Although the editors stress that "globalization involves more than economics" and assert that they do not want to completely reject realism since it "is still a useful guide" for understanding certain types of foreign policy behavior (pp. 5, 190), some chapters have the kind of single-minded focus on economic variables that realists allegedly have on the distribution of military capabilities in an anarchic state system. Whereas the passing of bipolarity has prompted realists and neorealists to speculate about the stability of various configurations of power that might arise by the end of this century, this fruitful line of inquiry is ignored by some of the authors who instead give a privileged analytical position to matters of commerce, capital accumulation, and investment.

Despite these differences, several themes connect many of the essays. The first is the rise of a system of global dominance based on an ideology of liberal democracy plus market. Whether called a "new orthodoxy" (Rizvi), an "ideological monopoly" (Sunkel), or "ideological catholicity" (Ake), various contributors submit that this doctrine and its embodiment in World Bank and International Monetary Fund structural adjustment programs are problematic. "It is the poor and the weak who bear the burden of this catholicity," writes Claude Ake: "Generally it does not reflect their interests and they cannot change it, nor can they afford to indicate that they dislike it" (p. 27).

The second theme pertains to the contour of the state system of the early twenty-first century. Many of the contributors envision a world divided between rich and poor countries, a division that marginalizes the impoverished, weakens their voice in global governance, and raises the specter of chronic instability and political rivalry. Among the most serious threats to the stability of the post–Cold War world, notes Zhang Yunling, will be "competition for regional supremacy, [and] the division of new nation-states" (p. 92). Yet according to Robert O. Keohane, the impact of globalization and the end of the Cold War will be to "reduce the incentives for major powers to maintain order in [these] zones of conflict" (p. 181). Conflicts that once would have precipitated great-power involvement will be left to run their course uninterrupted.

Finally, a third underlying theme in the book concerns the results from the dialectical interplay between globalization and what Michael Zürn calls "individualization" (p. 141). Both processes engender political integration, as well as political fragmentation. Globalization is thought to encourage integration by increasing international interdependence though it can promote fragmentation by presenting serious challenges to the way states have pursued their security and welfare goals. Individualization, which refers to the replacement of traditional orientations with individual cost-benefit calculations, also contributes to integration by supporting democratization; but it can increase fragmentation by creating new social cleavages. While on balance, globalization and individualization may have fostered political integration in core states, the contributors to the book suggest that they have bred political fragmentation throughout the periphery of the world system.

Alfred North Whitehead once quipped that "it is the business of the future to be dangerous." The contributors to *Whose World Order?* have done an admirable job in describing the dangers facing the world community as it approaches the next millennium. Regardless of whether one accepts the claim that the wrenching changes behind these dangers undermine much of mainstream realist theory, the book should stimulate reflection on the extent to which widely accepted generalizations in the field of international relations are context specific.

Boise State University GREGORY A. RAYMOND

International Institutions and the Political Economy of Integration. By Miles Kahler. Washington: Brookings Institution, 1995, 165p., $21.95.

The enormity of the literature on international organizations creates problems for an author seeking to synthesize that scholarship and draw out its policy implications. The approach taken by Miles Kahler in this slim volume (which comes packaged complete with its own book review, by Robert Keohane) is to focus on the challenge to international institutions posed by the integration of national economies. How, Kahler asks, are organizations and arrangements like the International Monetary Fund (IMF) and the General Agreement on Tariffs and Trade (GATT) coping with the strains posed by the integration of factor and commodity markets? What light do their responses shed on competing explanations for the existence and evolution of such institutions? What does the efficacy of those responses tell us about the merits of rules-based versus ad hoc procedures, encompassing versus issue-specific agreements, and global versus regional groupings?

Kahler's study is superimposed on a backdrop of pervasive disagreement about how the economic and political role of international institutions is best understood. Economists tend to regard them as solutions to contracting and commitment problems in the international domain. They arise, in this view, to disseminate information on the compliance of nations with the terms of international agreements, to levy penalties against signatories that renege, and to coordinate the terms of the bargain across countries. This functionalist approach has been challenged by political scientists who view such organizations as deriving their legitimacy from internationally accepted norms and understandings.

Such disagreements are compounded by the degree of institutional variation exhibited by international organizations, spanned by the formal, ostensibly rules-based IMF at one extreme and the more flexible, less structured GATT on the other. Lawyers—hostages, like the rest of us, to their training—regard formal, rules-based arrangements as the highest form of international organization, to be emulated by more primitive forms of political life. Economists see the organizational characteristics of international institutions as driven by the structure of transactions costs in the political and economic markets in which they operate. Where unanticipated contingencies prevail, institutions will be flexible; but where transactions costs are high and defection is crippling, rigid rules will dominate. Political scientists tend to regard international organizations as progressive institutions that serve the public good by containing the destructive tendencies of states in an anarchic neorealist world.

Though the fact is not clearly evident from its title, Kahler's book is concerned almost exclusively with economic arrangements (though the European Union, which comes in for considerable discussion, is clearly more than this). Its central chapters focus on the contrast between the GATT and the IMF and on the varieties of regional arrangements. Kahler characterizes the GATT as having been more successful than the IMF in coping with disturbances to the international system since World War II and over recent decades in particular. This he attributes to the inability of the IMF's relatively rigid structure to accommodate the unanticipated consequences of the increase in capital mobility that transformed the international monetary system of which it was steward. An organization established to manage a system of par values and avert the deflationary dangers of inadequate international liquidity was poorly positioned to address problems associated with floating exchange rates, inflationary finance, and unserviceable sovereign debts. The GATT, in contrast, utilized flexible procedures adaptable to changing circumstances, helping it smoothly to shift its attention from tariff to nontariff barriers and from trade in manufactures to trade in services.

Kahler is not unaware of the conjectural nature of these conclusions. There are problems, he acknowledges, with generalizing from what is essentially a sample of two. Moreover, while it is impossible to gauge the efficacy of IMF and GATT intervention in the absence of an explicit counterfactual, it is extremely difficult to envisage how international monetary and commercial relations would have evolved in the absence of the two institutions. Outcomes may have been less satisfactory in the monetary than the commercial domain not because the IMF has, in some sense, been less effective but because the problems for which it is responsible have simply been less tractable. And it is hard to understand why, if a pliant GATT-like structure is better suited to an environment of deep integration, policymakers should have decided just now to substitute a more formal World Trade Organization (WTO).

The proliferation of regional arrangements offers a source of additional evidence that may ameliorate at least some these methodological problems. Kahler provides a very useful review of the relevant alphabet soup, covering the EU, EFTA, NAFTA, APEC, and ANZCERTA. Regional arrangements, he concludes, are evolving toward a mode of "policed or monitored decentralization" involving broad areas of harmonization agreed upon internationally. Within these areas, mutual recognition and limited policy harmonization can coexist with a degree of national autonomy. In a world where international pressures penetrate deep into national economies, the effective harmonization of policies toward international transactions can be highly intrusive. Assigning extensive powers to an international organization would therefore create a structure that would be resisted by member states and rob the arrangement of credibility. The preference for limited GATT-like monitoring and policing follows from this insight. By implication, the IMF of the future is more likely to encourage modest policy coordination among countries than to enforce their participation in a rigid exchange rate arrangement like the pre-1971 Bretton Woods System. Similarly, the WTO is likely to bear a closer family resemblance to its immediate progenitor, the GATT, than to its distant relative, the stillborn International Trade Organization.

The European Union is at the same time a source of support and a challenge to the author's hypothesis of a trend toward monitored decentralization. The European Union vests significant decision-making power in its Council and Commission, a tendency that is reinforced by the Maastricht Treaty and the European Central Bank for which it makes provision. At the same time, the treaty and the Union, with their emphasis on subsidiarity, seek to cultivate local support through devolution. If the European Union is a model for other international institutions, it suggests that the question is not whether such arrangements will be centralized or decentralized but how different issues will be dealt with simultaneously, with different degrees of delegation, without undermining the coherence of decision making.

The European Union is the inspiration for many of Kahler's other particularly provocative insights and conjectures. He shows that the effectiveness of international institutions presupposes links to national interest groups, citing Europe's farmers and Common Agricultural Policy as an example of how such linkages can be constructed. He suggests that the success of the European Union owes much to its capacity to link issues of interest to different countries, a conclusion that is less than encouraging for international organizations that are function-specific and whose structure offers little scope for cross-issue trade-offs.

Kahler's book contains much more that prompts reflection about the prospects for effective governance of the global economy. It is hard to think of another volume on the subject that covers so much ground, both positive and normative, in such short compass. This one deserves a wide audience.

University of California, Berkeley BARRY EICHENGREEN

The Convention on the Rights of the Child: United Nations Lawmaking on Human Rights. By Lawrence J. LeBlanc. Lincoln: University of Nebraska Press, 1995. 337p. $45.00.

The Convention on the Rights of the Child is Lawrence LeBlanc's third major contribution to the human rights literature. The first, his 1977 book, *The OAS and the Promotion and Protection of Human Rights*, was based on his doctoral dissertation. In 1991, he published an outstanding study of *The United States and the Genocide Convention*. His newest book again fulfills the high standard established in his earlier work.

The Convention on the Rights of the Child has three parts: "Establishing the Convention," which provides the historical background; "Defining Children's Rights," which analyzes the content of the convention, and "The Committee on the Rights of the Child." The analysis of the convention's content, based on a general framework devised by Jack Donnelly and Rhoda Howard, distinguishes among *survival, membership, protection,* and *empowerment rights.*

The few criticisms that could be made about LeBlanc's presentation apply not only to his work but to the human rights field generally. For example, he sometimes speaks of the Committee on the Rights of the Child (CRC) as the implementation mechanism of the convention. However, Article 43(1) makes it clear that the committee is established for the purpose of "examining the progress made by States Parties in achieving the realization of the obligations undertaken in the present Convention." LeBlanc recognizes, in his conclusion, that "the primary responsibility for the implementation of the convention falls first and foremost to the legislative, administrative, and judicial institutions of the individual states that ratify it" (p. 286). Analysts of human rights should be consistent in describing the states parties as the primary agents for implementation of human rights conventions, not the treaty bodies in the United Nations. The UN treaty bodies such as the CRC are best understood as instruments of supervision and monitoring, that is, *accountability*, overseeing the states parties to assure that they carry out their responsibilities under the agreement. Much too often, people place blame for failures of implementation on the United Nations when the fundamental responsibility lies with national governments.

Also, there is an important omission common in the human rights literature. International governmental organizations such as the United Nations Children's Fund, the World Health Organization, the Food and Agriculture Organization of the United Nations, and the World Bank undertake activities that affect the human rights of children and others. These organizations are rarely held accountable with regard to international human rights law in any explicit way. It should be recognized, however, that these organizations are not autonomous. Though they may sometimes seem to act as if they had to answer to no one, they are in fact instruments of their member states. To the extent that these member states are accountable under international law, including international human rights law, these organizations should be held accountable as well.

LeBlanc reports on what representatives of the nongovernmental organizations have described as the "amazingly low level of protection" the convention gives to children in armed conflict situations. For most purposes, the convention defines children as persons under 18 years of age (unless, under local law, the age of majority is attained earlier) but makes an extraordinary, anomalous exception. Article 38 states that parties are to ensure that persons under 15 years of age do not take a direct part in hostilities and are to refrain from recruiting anyone under 15 to serve in the armed forces. Thus, Article 38 makes a point of allowing children under 18 to take direct part in hostilities and to be recruited into a nation's armed forces. With support from Canada, the Soviet Union, and the United Kingdom, the United States played a leading role in insisting on this shameful exception.

It would be most useful if LeBlanc, using his earlier book, *The United States and the Genocide Convention* as a model, now did a detailed study of the United States' performance relating to the Convention on the Rights of the Child. As he points out, the United States' peculiar behavior showed up early in the negotiating process when it blocked consensus on the financing of the working group and forced the decision to the General Assembly itself. During the negotiations, the United States insisted not only on retaining the right to recruit children under 18 into armed forces but also insisted on the right to execute people who had committed capital crimes before the age of 18. Only six countries executed minors in the 1980s (Bangladesh, Iran, Iraq, Pakistan, and the United States, and Barbados), and the latter has since abolished capital punishment for people under 18 years of age. While LeBlanc provides a good deal of information on the United States' negotiating positions on a variety of issues, he does not describe its position on some important issues such as the right to education.

The United States could ratify the convention with reservations about service in armed forces, capital punishment, or other issues; but it has not. Children's advocates in the United States were pleased when Bill Clinton was elected president, especially because Hillary Clinton had been active with the Children's Defense Fund. The advocates expected that the convention would be signed early in the Clinton administration and were deeply disappointed when it was not. It finally was signed on 16 February 1995 by U.S. Permanent Representative to the United Nations Madeleine Albright. However, rather than serving as a great cause for celebration, the signing seemed done in stealth, with no notice in the press, almost as if the administration was ashamed that it had signed it.

At the beginning of the Clinton administration, the prospects for ratification of the convention were as good as they have ever been; but the prospects under the now Republican-controlled Senate are dim. The United States' performance with regard to children, both its own and the world's, needs to be examined more closely. As LeBlanc shows, the RC constitutes an important advance in the strengthening of human rights around the world. It is a tragedy that the United States has so frequently pushed in the opposite direction.

University of Hawaii GEORGE KENT

The Great Powers and Global Struggle, 1490–1990. By Karen A. Rasler and William R. Thompson. Lexington: University Press of Kentucky, 1994. 275p. $35.00.

The last decade-and-a-half of historical-structural studies (including those on long cycles) saw two insights established among the basics of world politics: the existence and the centrality (over the past half-millennium) of the role of global leadership and the distinctiveness of global (or systemic) wars—of which there have been only a handful (maybe only four or five) over that same time span.

The authors take these insights as the premises of their analysis and ask, What explains these global wars, and how were global wars related to the rise and decline of global leadership? To answer these questions, they construct a causal "structural-transition" model of close to 30 variables. But the crux of it is the relationship between leadership and challenger, which they interpret, in the relevant time span, as one between global (or naval) power and regional (or West European army) power. They test parts of the model (among eight of the variables) and statistically confirm a number of hypotheses, such as the relation between naval and army concentrations (that, interestingly, they find to be alternating in their peaks); and they also dismiss the significance of others such as causal connections between overconsumption, overextension, and "decline." They conclude that global leaders' concern to suppress threats to the central region before they become global threats was and is critical to understanding world politics and that it is too early to write off transitional (global) warfare among the great powers: "Its demise within the next generation or two is less than a sure thing" (p. 191).

This work is notable for among other things, its careful attention to a number of literatures, an eclectic approach to theory building (the authors co-opt the Organski-Kugler transition model, and give it a new test), and a sustained concern for the empirical validation of propositions—in part, with systematic evidence of their own collection. Of value are Appendix B, "On Identifying Systemic Wars" and the new data on West European army sizes, clearly showing four peaks (Spain, France I and II, and Germany) and forming a clear counterpoint to the Modelski-Thompson series of seapower concentration. This makes it possible to draw a clear contrast between the role of global leadership and that of challenger rooted in the central region and to present a new perspective on regional politics, thus raising the important question, Is the central region of world politics, which in the past half-millennium resided in Western Europe, now moving to East Asia?

This is a carefully crafted book, and repays close study, even if some of it is not light reading. While much of this has already been presented at conferences and published in international relations journals between 1988 and 1994 (and some will be familiar to specialists), care was taken to integrate the material, and the work as a whole represents a significant contribution to historical-structural world politics and to the long-cycle research program. As with all good research, it also points the way toward more work to be accomplished. To begin with, the model needs testing not just against the West European episode of international affairs but also against what went before it and what has followed it in global politics (Thompson has already begun to write about the entire past millennium as a unit). Also, should not the analysis be extended to all world regions, and not just to the central one? Might there not be room, among the variables, for democratization? Could not the structural-transition model be made more parsimonious, maybe in such a way that it begins to explain not only one case at a time of transitional warfare but also a sequence of transitions with or without warfare that might be shown to be evolutionary?

University of Washington GEORGE MODELSKI

The Fragmentation of Afghanistan: State Formation and Collapse in the International System. By Barnett R. Rubin. New Haven: Yale University Press, 1995. 378p. $34.95.

The Soviet occupation of Afghanistan was undoubtedly one of the decisive events in the history of Cold War, and the burdens of that occupation played at least some role in triggering the collapse of the USSR and the communist system more generally. Yet the Afghan conflict remains poorly understood by scholars, despite the extensive coverage it received and its obvious importance. Too many studies, even in scholarly venues, have been marred by political bias and Cold War passions. In this context, Barnett Rubin's study is a refreshing departure and provides the first serious book-length effort to place the Afghan conflict in a social-scientific perspective.

Rubin seeks to explain the anarchy and de facto fragmentation that have plagued Afghanistan since the demise of the indigenous communist regime in 1992. An extensive survey of Afghanistan's sociopolitical development is presented, focusing on the past twenty years. Though it is difficult to capture the full complexity of Rubin's analysis here, the basic argument may be summarized. It was the excessive autonomy of the Afghan state along with the concomitant fragmentation of the society into discreet social units separated from each other by ethnicity, language, religion, and geography that weakened the structure of central authority and ultimately led to the collapse of state power. The study provocatively argues that the communist regime, which seized power in 1978, was in certain respects a continuation of the traditional Afghan state structure and that the communists' "revolutionary" project only exacerbated longstanding institutional weaknesses. Rubin sums up the problem as follows: "The autonomy of the state made it relatively easy for a small, relatively well organized, highly ideological group [i.e., the Afghan communists] to seize the state and hold it for a time. But autonomy also meant that the party in power had virtually no resources other than violence for implementing its program, and society had few other means of resistance" (p. 120). When the communist regime finally collapsed in 1992, virtually all institutions of central governance collapsed along with it. The victorious Mujahiddin guerrillas were too divided among themselves even to begin the process of reconstructing these institutions, and the outcome has been the distressing "Hobbesian state of nature" that characterizes Afghan politics at present. Though not stated explicitly, there is an important causal argument in this book, one that will interest a wide range of scholars: State structures that are characterized by exceptionally high levels of autonomy and separation from the societies that they govern tend to be those that are vulnerable to collapse.

Overall, this study is a major contribution to the literature on state collapse and disintegration (and a very timely one as well, given recent events in such places as Somalia and ex-Yugoslavia, as well as Afghanistan). The book is obviously the result of many years of intense research and relies, in part, on hard-to-obtain field data from such sources as the United Nations High Commission on Refugees, the Swedish Committee on Afghanistan, and a CIA-connected consulting group. There are also extensive references to foreign language publications, including many in Farsi, as well as French and German. The book is replete with original data pertaining to Afghanistan, much of which will prove invaluable for future studies; and for the most part, a high standard of scholarship is maintained.

However, there are also some flaws. The theoretical analysis is sophisticated and insightful, but it lacks a clear focus and does not adequately distinguish theories that are confirmed by the Afghan case from those that are disconfirmed. The book presents a one-sided analysis of the rural sector, mistakenly arguing that there was a relatively egalitarian distribution of land in prerevolutionary Afghanistan. Rubin writes that "large landholdings in Afghanistan were rare" (p. 119), while ignoring widely cited statistics that 2.2% of farm households held some 42% of all arable land. Rubin is surely correct when he notes that the communist land reform of 1978–79 was characterized by incompetence and brutishness, but he errs when implying that land reform was altogether unnecessary. The study generally avoids journalistic clichés, but these, unfortunately, do creep into the narrative from time to time (e.g., when the term *drug cartels* is used [p. 263] to describe intensely competitive drug gangs that obviously had not been cartelized). The bibliography cites a number of works (including a sensationalist account by the editor of the *Ladies Home Journal*) that seem inappropriate for a book like this, while it omits such important studies as Alfred W. McCoy's work on opium/heroin trafficking and Valentine Moghadam's research on Afghan gender relations. Rubin presents a lengthy and useful discussion of the atrocities committed by Afghan communists (a subject on which Rubin is exceptionally well informed); but he understates the record of atrocities committed by the Mujahiddin guerrillas, and he ignores altogether the widespread practice of torture, which was used to enforce internal discipline in certain guerrilla groups. While there is extensive, diffuse discussion of gender issues, this discussion is not always even-handed; and one significant point is omitted: most of the Mujahiddin groups held retrogressive views regarding the social status of women. Such omissions were understandable while Afghanistan was under Soviet occupation, but they seem more questionable now.

Despite these shortcomings, *The Fragmentation of Afghanistan* is a first-rate study, easily one of the best to appear on this vitally important subject. It is essential reading for Afghan specialists, and it also will enrich discussion in several areas of comparative politics and political sociology.

University of Arizona DAVID N. GIBBS

Soldiers, Society, and National Security. By Sam C. Sarkesian, John Allen Williams, and Fred B. Bryant. Boulder: Lynne Rienner, 1995. 204p. $22.95.

As the size of the U.S. military becomes smaller and the threats and missions for which it is charged change, it is an interesting and important matter to examine how these changes will affect the professional military that will remain in place as change occurs. That is the broad task that the authors of this slender volume have undertaken. The major tool of their analysis is a series of interviews with professional officers, which are aggregated and presented, mostly as anecdotal evidence to demonstrate points being made.

The authors begin their study from four assumptions that are then combined in a major research hypothesis. The "components" of the approach, as they put it, are (1) that the changing military environment requires new force structures and strategic dispositions within the military, (2) that a degree of separation must be maintained between the military and civilian societies, (3) that the civilian political and professional military professions must be reconciled with one another, and (4) that military "professional norms and ethos" must be compatible with the first three. This leads to their hypothesis that "a cohesive military, fine-tuned to respond to contingencies across the spectrum, can be achieved only by the melding of the key substantive elements of each of the four components into a reasonably coherent whole" (p. 2).

These statements set the direction for the book. The argument begins with the assertion that a changed environment means that the military establishment will have to operate in ways, and for purposes, for which past experience may offer few reliable guides. The army is the focus of this observation and of the study generally, because it is the army's missions that are most fundamentally affected by change. More specifically, the challenges of the future are mostly in unconventional situations in the old Third World, for which preparation for a major land war in Europe may offer scant instruction.

The question is how to effect the transition—in essence, how to educate the professional military so that it can function effectively in the new environment. To look at that question, the authors present two partially contradictory perspectives about the problem: (1) the nature of military life and the need to keep some separation between the military and civilian sectors and (2) the desirability of providing advanced, graduate-level education to military officers in civilian universities.

The authors contend that military professionals are different from civilians and that some of the difference is necessary. The basic source of difference is that soldiers potentially put their lives on the line as part of their profession, what they call "ultimate liability." This makes military people an inherently conservative force imbedded in a liberal society that they are honor-bound to defend but can only emulate at potential costs to their effectiveness as military professionals. The problem, however, is that insulation from civilian society may leave the professional military underprepared to understand and compete in the competition over new roles and dwindling resources.

How can the military become competitive with simultaneously becoming less military? The authors' answer is that a larger number of military professionals going through the professional military education (PME) system should be assigned to civilian graduate schools, rather than the traditional war colleges that have been the tickets to career advancement. The authors praise the war college system (overlavishly, in the opinion of this reviewer, who has taught in the professional military education system of all three services) but argue that an alternate—and broader—set of perspectives can be gained by putting officers into regular graduate schools.

The two perspectives potentially contradict one another in that the civilianization of professional graduate education almost certainly erodes the sense of difference held by military people who go through civilian graduate schools. That may be fine (I would argue it is), but confinement within the PME system does ensure that those who go through it remain reasonably "pure" in their orientation. The price of such purity (the authors could have said this more explicitly) is that officers need civilian exposure, and the war colleges do not offer the equivalent level and quality of education as are offered in graduate schools.

I had two relatively minor quarrels with the authors. One was their tendency to treat their data base of interviews as a source from which quotations supporting particular points could be found without any real attempt to demonstrate that these quotes were representative of the universe. The other quibble is that the authors, in their statistical analysis in the appendices, do not control their sample either for service or for military operational specialty within services. I suspect one would find some interesting differences in attitude between, for instance, army and navy officers and also between special forces and field artillery officers within the army.

The advocacy of civilian graduate education is the real underlying theme of the book, although it appears sporadically through the text (primarily in the first and last chapters). Much of the real value of the work lies in between, in the trenchant analysis of the changing environment and the challenges they pose for the professional military. From personal observation, this is an important and understudied problem, particularly within the military itself. Those charged with developing curricula within the PME system can gain from this book. At the same time, this perspective is of value to anyone actively seeking to understand how environmental change in the wake of the Cold War affects an important American institution.

University of Alabama DONALD M. SNOW

Security, Democracy, and Development in U.S.–Latin American Relations. Edited by Lars Schoultz, William C. Smith, and Augusto Varas. Miami: University of Miami, North-South Center, 1994. 284p. $29.95.

What role should the military play in post–Cold War Latin America? What are the major security issues facing the hemisphere, and what kind of interstate arrangements should be developed to deal with them? In raising these questions, this timely and informative collection of essays brings to light the multitude of opportunities and challenges presented by the current conjuncture for reshaping civil-military interactions within Latin American nations and for redefining security arrangements between the United States and Latin American governments. What ultimately lies at stake is whether Latin America can break the cycle of military interventionism and U.S. domination that has plagued the region in the past. What this book does best is highlight the possibilities for improved relations along these domestic and international fronts, while simultaneously identifying underlying tensions that could erupt and strain (if not reverse) current tendencies toward cooperation. What differentiates this volume from similar works is its provision of prescriptions for policymakers charged with establishing new roles, relationships, and institutions appropriate to changed domestic and international conditions.

Security, Democracy, and Development in U.S.–Latin American Relations features 11 essays that explore the process of restructuring currently underway, underscoring the uncertainties, possibilities, and pitfalls that the future holds. The method of pairing North Americans with Latin American scholars on similar subtopics helps to ensure the presentation of perspectives from both regions. Varas and Schoultz persuasively depict the current era as a sea-change. Schoultz underscores two factors bound to bear critically on U.S.–Latin American relations: Latin America's increased ability to affect U.S. security through immigration and drugs and the unprecedented importance of domestic public opinion in the formulation of U.S. foreign policy. Varas usefully reminds readers that the North American emphasis on the "diminished threats" facing Latin America is not shared by many Latin Americans, who focus instead on the assault that the neoliberal global order has wrought on state sovereignty.

Informative and well-reasoned essays by Nina S. Serafino, J. Samuel Fitch, Juan Gabriel Tokatlian, Thomas Guedes da Costa, Felipe Agüero, Carlos Acuña, and William C. Smith examine recent developments in inter-American military relations, as well as civil-military relations within Latin America. Their shared view of the now low probability of military seizures of power is tempered by other troubling trends to which they point. Fitch foresees the rise of nationalism within Latin American militaries in reaction to the collapse of the threat that had united them with the United States, coupled with resentment against the United States for pushing "operations other than war" at the cost of conventional defense missions for the region's armed forces. For the case of Colombia, Tokatlian confirms a similar possibility among civilian actors. Agüero and Guedes da Costa draw attention to the resumption, in some countries, of nation-building/developmental tasks for the military reminiscent of the 1950s and 1960s, during which military influence expanded dramatically. Serafino emphasizes that the U.S. armed forces have their own interests (that often contravene the political directives of the foreign policy establishment) in maintaining relations with Latin American militaries. Acuña and Smith demonstrate that the domestic political power of the military affects even the continued capacity for weapons production, an issue of broader international relevance.

Klares foresees friction on the issue of arms control, as the United States tries to reign in countries whose policies of proliferation in days past were insulated by their anticommunism. Rojas argues convincingly for the construction of a cooperative security regime in the hemisphere. Mares offers an interesting conceptual scheme for thinking about the form that intra-Latin American and inter-American security relations may assume.

The unifying exhortation is that U.S. and Latin American policymakers not allow the current context for change to slip away before designing policies aimed at reversing or preventing past ills. The enterprise of this volume is by intention largely speculative. Nevertheless, many essays would have been strengthened by going beyond reflection and showing how matters are actually shaping up. The volume could also have benefited from a final integrating chapter, synthesizing the findings of the preceding chapters and pulling together their conclusions. Nonetheless, *Security, Democracy, and Development in U.S.–Latin American Relations* provides interesting theoretical coordinates for future research and is well worth reading.

Vanderbilt University WENDY HUNTER

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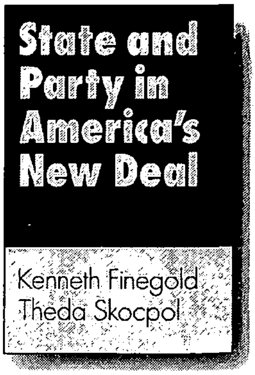
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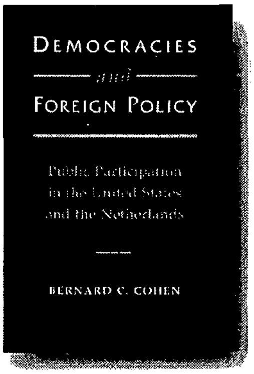
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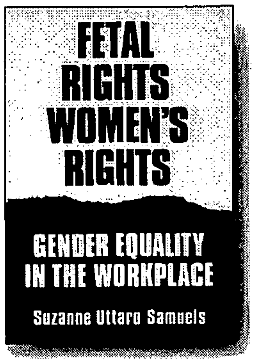
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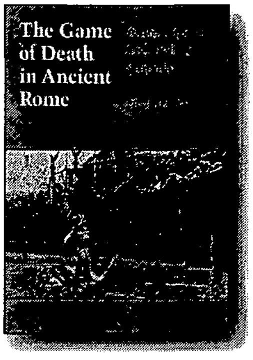
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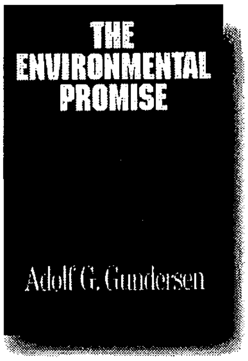
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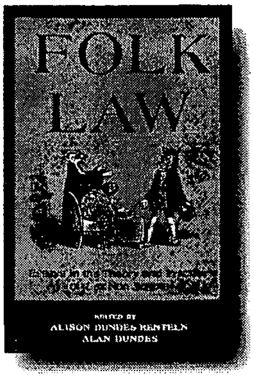
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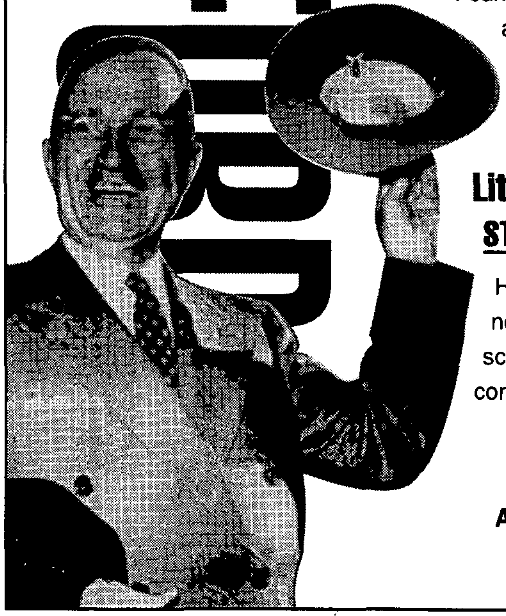

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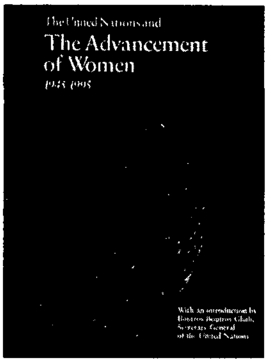

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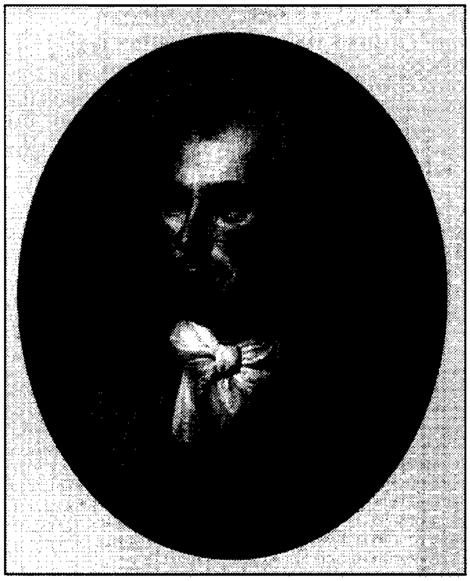

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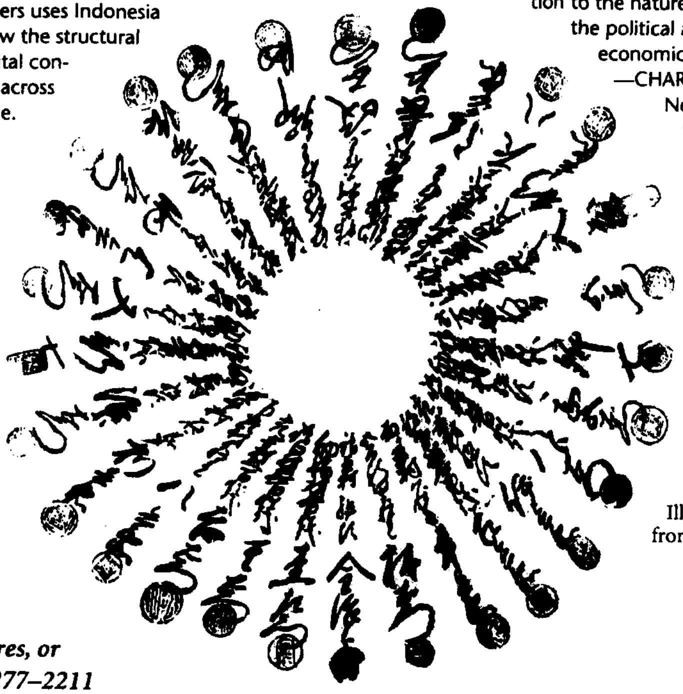

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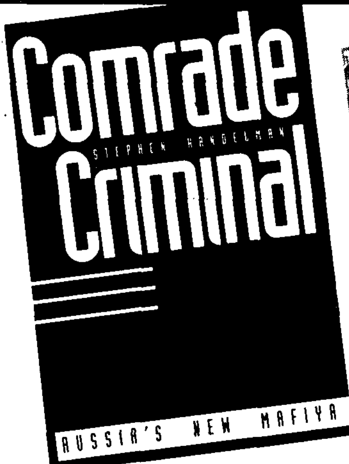
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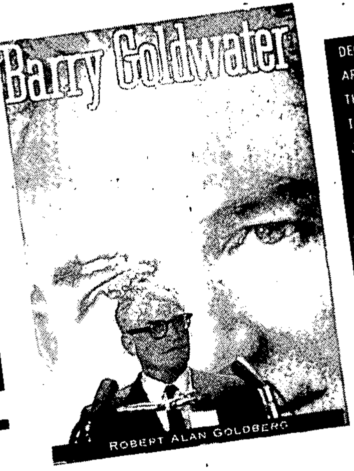
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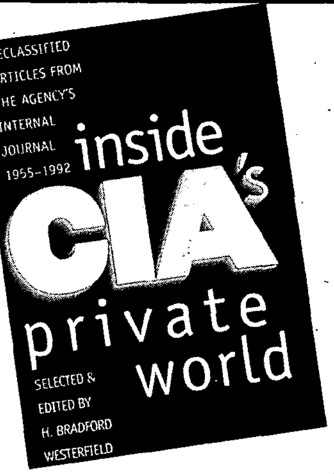
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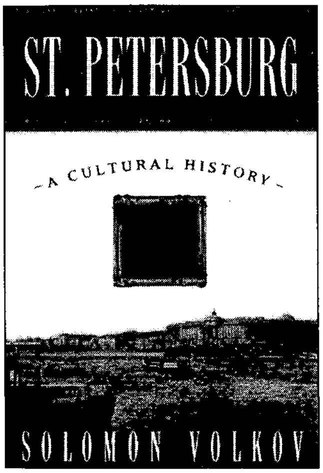

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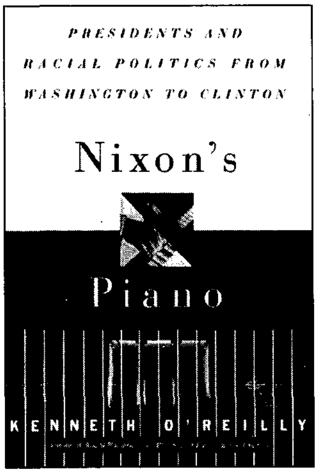

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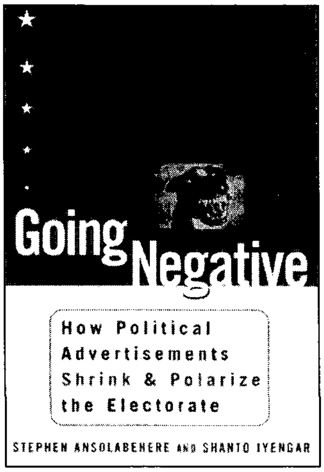
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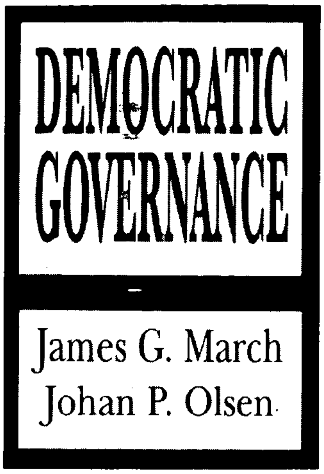

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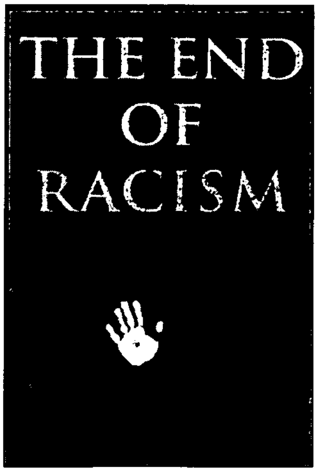

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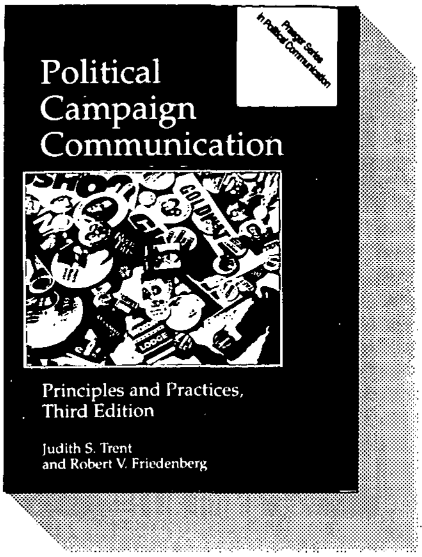
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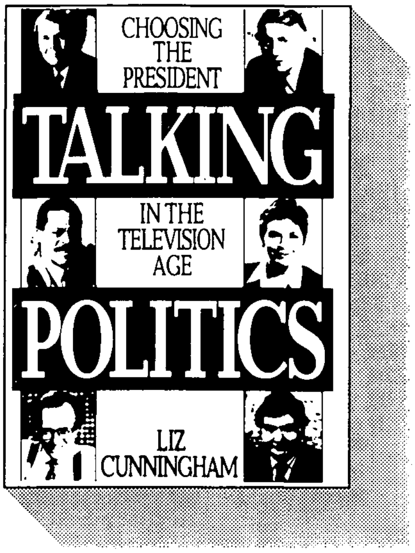

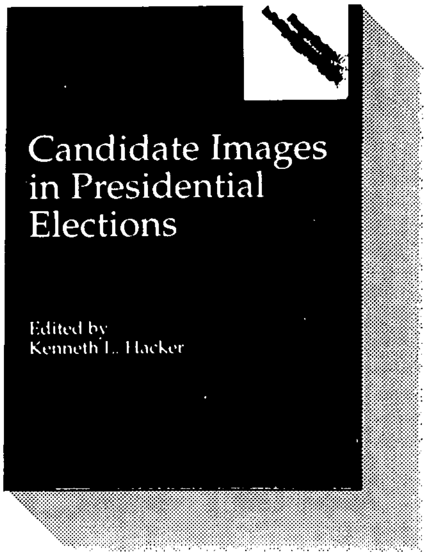
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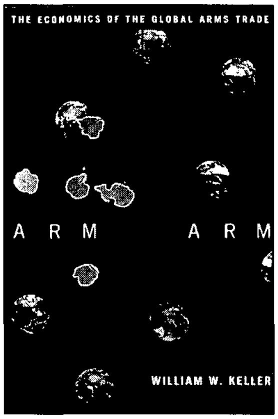
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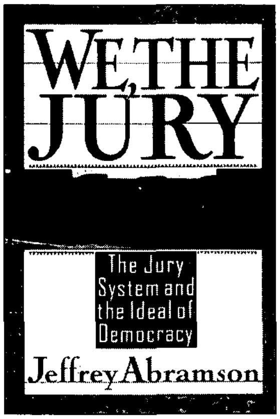
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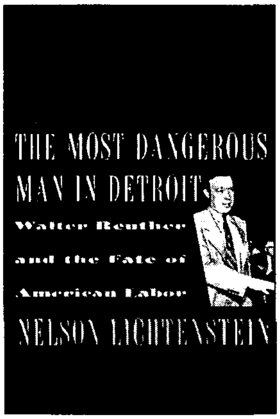
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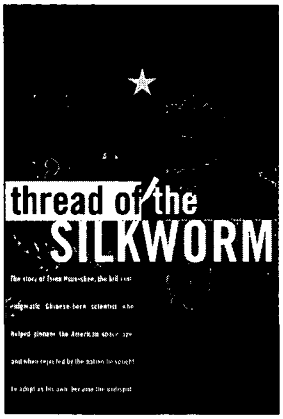
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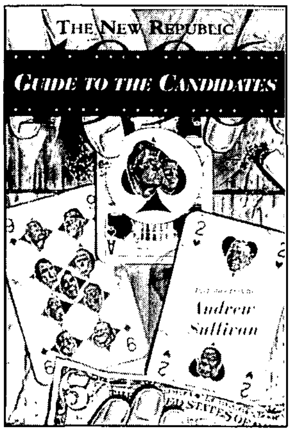
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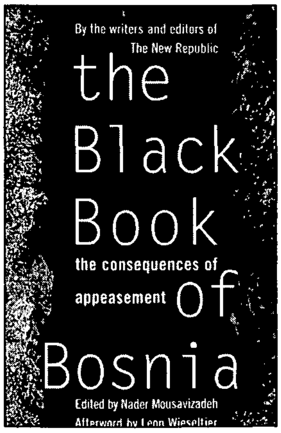
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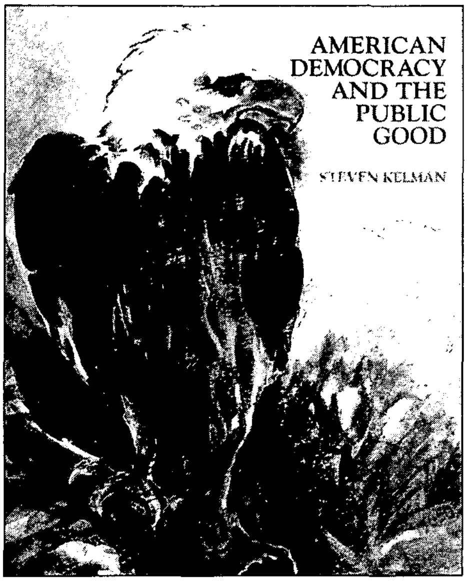

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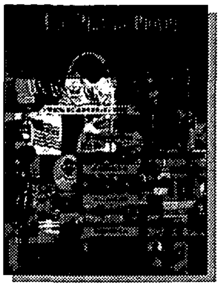

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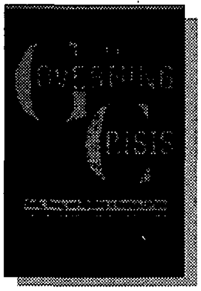

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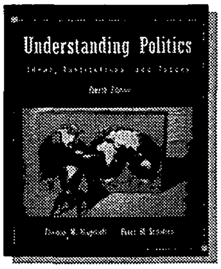

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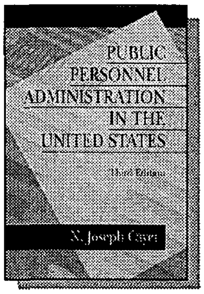
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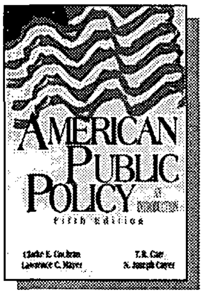
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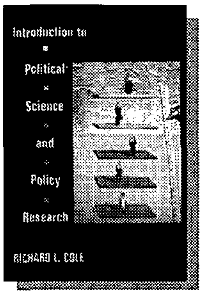
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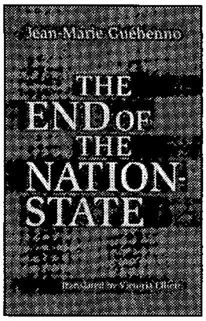

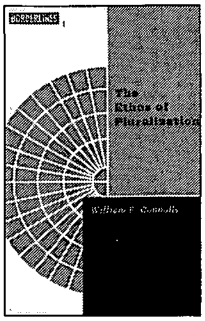

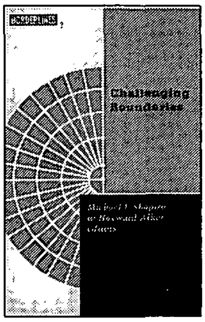

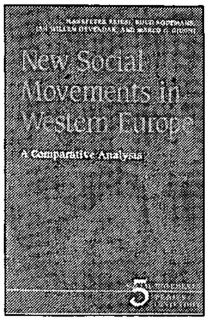

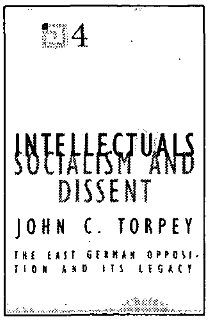
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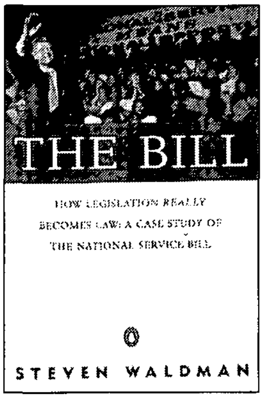

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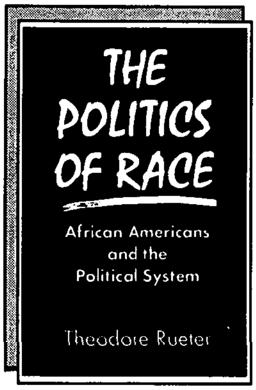

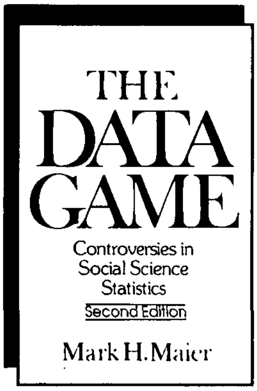

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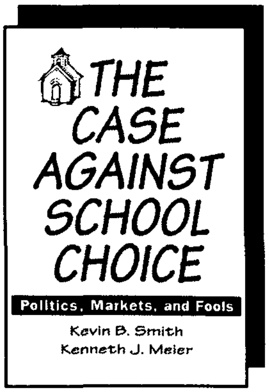

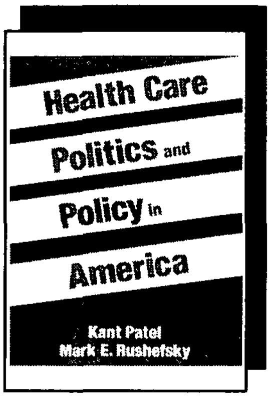

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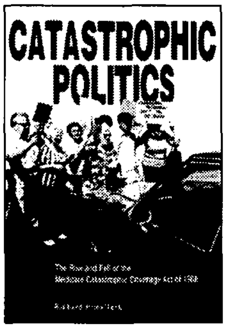

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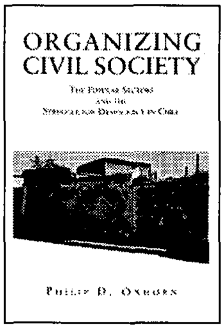

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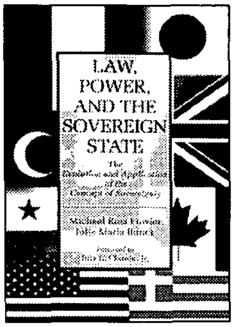

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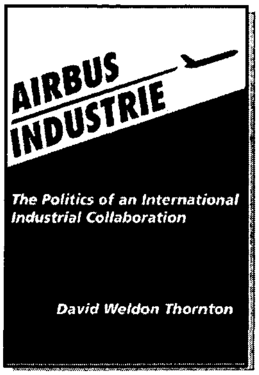

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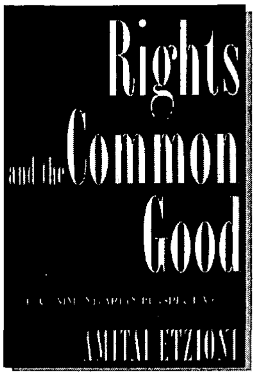

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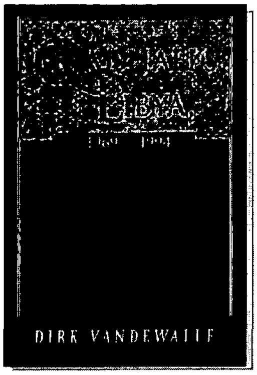

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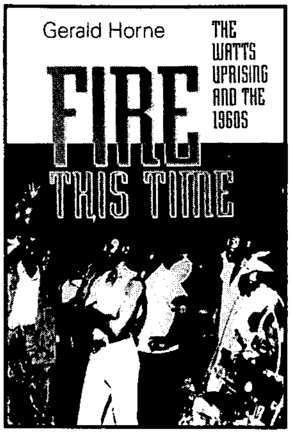

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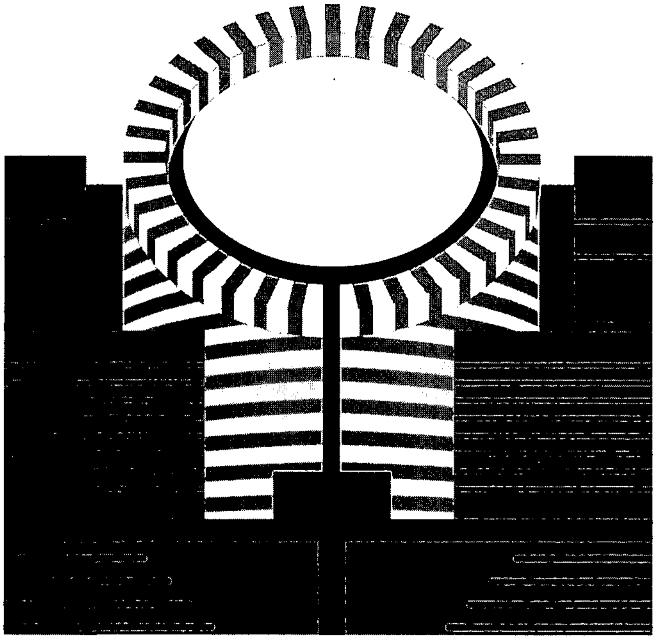

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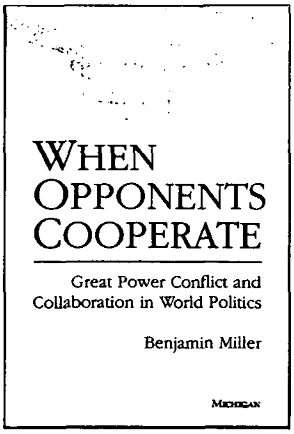
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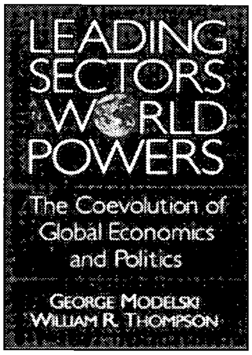
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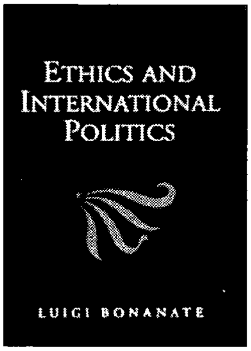
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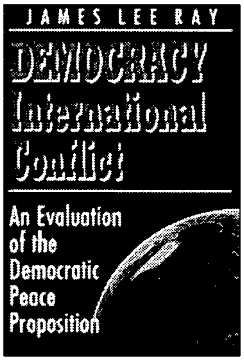
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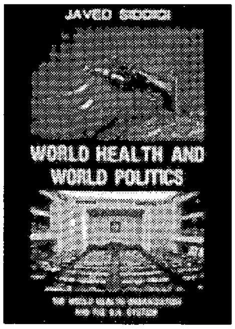
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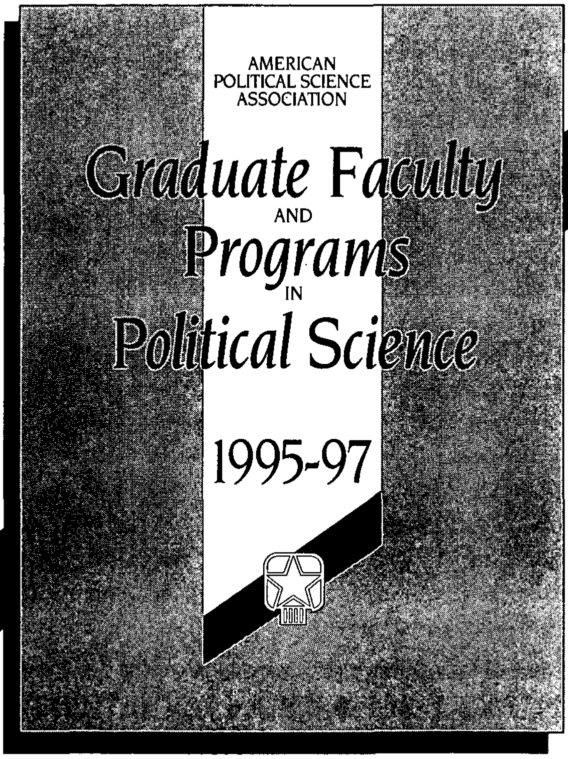
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